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ON

THE ORIGIN OF SPECIES

BY MEANS OF NATURAL SELECTION,

OR THE

PRESERVATION OF FAVOURED RACES IN THE STRUGGLE FOR LIFE.

By CHARLES DARWIN, M.A.,

FELLOW OF THE ROYAL, GEOLOGICAL, LINNÆAN, ETC., SOCIETIES;
AUTHOR OF 'JOURNAL OF RESEARCHES DURING H. M. S. BEAGLE'S VOYAGE ROUND THE WORLD.'

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1859.

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Bert James Loewenberg

THE MOSAIC OF DARWINIAN THOUGHT

THE DARWINIAN REVOLUTION established the hypothesis of transmutation; the establishment of the hypothesis of transmutation of species widened the scope of scientific method and absorbed biology within the realm of objective science. Darwin, in Kant's prophetic phrase, was "the Newton of the grass blades," for Darwin accomplished for biology what Galileo and Newton had already accomplished for mechanics and physics. The revolution in biology, spurred by Darwinism, was premised on a new way of looking at nature and a new way of looking at life. It was likewise premised on newer concepts of man which remolded the profile of humanity and therefore the cast of society. New views of life, of nature, and of society reshaped the metaphysics of the ultimate and reformed the philosophy of the temporal. The mosaic of Darwinian thought transcends Charles Darwin. The implications of Darwinian thought are wedged into a pattern which goes back to the Greeks and stretches into the future.

Darwin's father was never burdened with problems of precocity. It was the lack rather of standard achievement that punctured parental pride. Neither medicine nor theology was able to enlist the enthusiasm which random collecting and precise shooting evoked in his son. Charles Darwin, always respectful and affectionate, was nevertheless

without direction. While he had no irresistible urge for Holy Orders, the life of a country parson was not without its appeal. Episcopal priests in Victorian England were often more devoted to geology than to theology and as frequently regarded the one as divine as the other. The Reverend J. S. Henslow, Darwin's mentor at Cambridge, enjoyed rocks, animals, and flowers and accounted the inspiration of the Thirty-Nine Articles his greatest blessing. It was Henslow, natural philosopher and natural theologian, who first truly stirred Darwin. The quiet excitement of Henslow's brimming life and the uninhibited response he always gave to the needs of others opened Darwin's mind. Henslow's example robbed the conventional of fascination; thereafter Darwin's notions of contentment went beyond the country gentleman's world. Darwin's notions of contentment thereafter included the serious study of nature's world. Henslow helped Charles Darwin to discover himself; the *Beagle* experience helped Darwin to discover the meaning of science. Henslow and the *Beagle* shaped the outlines of Darwin's future. The first committed Darwin to the study of science; the second committed Darwin to the exploration of species.[1]

When Darwin returned from the *Beagle* voyage he had a clearly defined purpose. His larger aim was the study of science, his specific aim was the study of species. Science and species merged into the study of natural change, of which natural history was the larger framework and species the exemplification. Darwin proposed to investigate alterations in the whole of nature; he did not propose to study nature whole. Nature was to be taken apart piece by piece and its segments put together again conceptually. Darwin chipped away at the timeless rocks and dug the earth in search of ancient records of life and death. He observed plants and animals under domestication and investigated the geographical distribution of living forms as a clue to variation. He studied the fertilization of flowers and the behavior of earthworms and pondered over the probable meanings of geological subsidence and elevation. Whether he experimented or speculated, collected or dissected, his goal was the same. The habits of the bee, the life of the ant, homology, taxonomy, embryology were excursions into the dynamics of natural change, essays into the life history of species which was the history of life.[2]

[1] Bert James Loewenberg, ed., *Charles Darwin: Evolution and Natural Selection* (Boston, 1959), pp. 9, 12-13.
[2] Francis Darwin, ed., *The Life and Letters of Charles Darwin*, 3 vols. (1887), I, 62, 68, 71-73. (Henceforth referred to as *Life*.)

Darwin borrowed ideas wherever he could find them. He relied on Sir Charles Lyell, whose prestige as a geologist was established even before Darwin embarked on the *Beagle* and whose famous *Principles of Geology* was among the carefully selected necessities Darwin crammed into his tiny cabin. From Joseph Hooker, Director of Kew Gardens, he drew the nutriment of friendship, a friendship documented by a life-time correspondence itself a chapter in the history of Darwinian thought. If the Darwin-Hooker correspondence records a fruitful interchange of ideas, the correspondence between Darwin and Asa Gray, Harvard botanist, was one long scholarly argument on the species question. Thomas Henry Huxley nominated himself Darwin's bulldog, but he was also Darwin's lynx-eyed critic whose sharply posed queries and neatly posed summaries made Darwin think and then think again. K. E. von Baer, a founding father of modern embryology, provided Darwin with a massive body of evidence and a subtle suggestion that fertilized his hypothesis. Alfred Russel Wallace, co-discoverer of natural selection, complemented his research and Thomas Malthus gave Darwin a dramatic illustration that leavened his thought.[3]

Darwin not only borrowed ideas, he eagerly sought them. He acted on suggestions brought to his attention in ways that later astonished those who made them. It was Darwin's uncle, Josiah Wedgwood, who first mentioned the relation of the action of worms to the sinking of ashes, cinders, and marls into the earth. The British Vice-Governor of the Galapagos told Darwin about the variation of the tortoise in the islands. French naturalists informed him of the domestication of the *Columba liva,* others of the insect-catching proclivities of the species of sundew, and others again of insects that clung to the leaves of certain plants and flowers. Darwin's writings, letters and monographs alike, are littered with references indicating his indebtedness to the knowledge of the past and to the learning of contemporary scholars. But no one save Alfred Russel Wallace and Charles Darwin created a theory of natural selection.

Borrowing is a commonplace of scholarship. But Darwin's adaptation of other men's ideas are subtly imbedded in the Darwinian mosaic. Like other students Darwin was enormously aided by the common fund

[3] *Life,* II, 146, 221; Gavin de Beer, ed., *Charles Darwin and Alfred Russel Wallace, Evolution by Natural Selection* (Cambridge, 1958), p. 17; Francis Darwin and A. C. Seward, eds., *More Letters of Charles Darwin,* 2 vols. (1903), I, 130-131.

of knowledge. But the weaving of data and concept together in newer conceptual relations was the mark of his greatness. The combination of the borrowing of data and idea and the weaving together of fact and concept discloses converging lines of thought. They also disclose his insatiable need for inspiration and confirmation, for fresh uncoordinated data. They likewise reveal the configuration of dissent, the form of approval, the mainsprings of Darwin's inner mind. Those from whom Darwin borrowed often failed to see Darwin's point, and those who perceived Darwin's intent frequently objected. Among them were those who, having adopted Darwin's view in certain particulars, called a strident halt at other crucial points. Hooker disputed and badgered, Gray accepted with qualifications, Lyell limited and hedged in edition after edition, and even Wallace, who out-Darwined Darwin in many a particular, uttered a resounding nay at a juncture Darwin could only regard as central.

"It seems truly remarkable," wrote one of Darwin's scientific friends apropos of Lyell and others, "how so many . . . could have failed to see their right road sooner."[4] Such comments were unquestionably encouraging, but they deepened Darwin's misgivings. His recognition of the difficulties of derivation and the candor with which he presented them in the *Origin* and elsewhere represents more than unsullied honesty and deference to doubt. Darwin's fears created an ambivalence and blurred his vision about himself and his work. These ambivalences partly explain his extreme caution of statement and the repeated deferral of publication plans. They also explain the care with which he set forth the objections to his conclusions, for he was also stating them to himself. Darwin had doubts of his own clarity of thought. He had even greater doubts of his powers as a thinker.

Darwin suspected his own convictions. How could he be so certain of his own correctness when others were so certain of his error? "The difficulties on my notions are terrific," he told Huxley, who had already compiled a *dossier* of quandaries to which Darwin could only reply: "I entirely agree with you."[5] The very fact that he was able to fit the conclusions of others so neatly into a design of derivation made him alternately exuberant and doubtful. To the physiologist, Wm. B. Carpenter, he confided: "When I think of the many cases of men who studied one question for years, and have persuaded themselves of the

[4] Hewett C. Watson to Darwin, 21 Nov. [1859], *Life*, II, 226.
[5] Darwin to Huxley, 2 Dec. [1860], *Life*, II, 354.

truth of the foolishest of doctrines, I feel sometimes a little frightened, whether I may not be one of these monomaniacs." He expressed similar apprehensions to Lyell: "thinking of so many cases of men pursuing an illusion for years, often and often a cold shudder has run through me, and I have asked myself whether I may not have devoted my life to a phantasy."[6]

Notwithstanding these apprehensions, Darwin was confident of the overall validity of his thesis. "I have," he wrote Huxley, again admitting the extent and gravity of the objections, "far more confidence in the *general* truth of the doctrine than I formerly had."[7] But it was just this confidence in the general truth of the doctrine which collided with canons of scientific method and gave Darwin the most prolonged pause.

Before the appearance of the *Origin*, Darwin's data was not yet compelling, but it was arresting and more than adequate to justify further testing of the hypothesis. And it was the hypothesis and the degree to which it satisfied such observations as he had been able to make that sustained his energy and enthusiasm. But it also strengthened his feelings of irresolution. He pondered, he vacillated about publishing his results, and continued with his work. He gathered information, he performed experiments, he asked everybody questions and tested his developing ideas against the reservations of his friends and his critics. In 1842 he wrote out a Sketch and in 1844 an essay, really a book, and doggedly continued the search. Had it not been for the impact of Wallace, he might well have delayed far beyond 1858. Neurotic indecision did not impede him. It was the conviction that so heterodox a theory required the most elaborate logical cogency and the fullest factual documentation. The impact of Wallace represents more than anguish over priorities. If Wallace's unanticipated discovery dismayed Darwin, it also bolstered his unflagging doubt. Someone else, looking at a different set of facts, had independently arrived at a synthesis supporting his own.[8]

The Darwinian evidence has been variously appraised. On the one hand, it is argued that respectable data for a provisional transmutation theory existed long before 1859. On the other hand, it is argued that Darwin's data presented in 1859 was inadequate. If the first assumption

[6] Darwin to W. C. Carpenter, 19 Nov. [1859], *Life*, II, 223-224. Darwin to Lyell, 23 Nov. [1859], *Life*, 229.
[7] Darwin to Huxley, 2 Dec. [1860], *Life*, 354 (italics in original).
[8] Bert James Loewenberg, *Darwin, Wallace and the Theory of Natural Selection* (Cambridge, Mass., 1959), pp. 16 ff.

is reasonable, Darwin's hypothesis was a late development, whatever date is assigned for its maturity. If the second assumption is reasonable, the factors inducing opposition to Darwinism must be differently appraised. The primary question then becomes why Darwin was opposed at all. But if Darwin's evidence was inadequate, the opposition explains itself and the reasons for Darwin's triumph become questionable. But if good evidence for transmutation existed before Darwin, the problem of Darwin's precursors requires reassessment. In any case these two versions of the argument for transmutation cannot be held simultaneously without careful qualification and analysis.

Darwinian data was deficient in completeness rather than in soundness. Completeness is relative, and the assertion of weakness without comparison is distortion. Paleontology, for example, did not actually come of age until the nineteenth century when William Smith (1769-1838), a civil engineer, noted the relation between sedimentary rocks and fossils. Fossils for the most part had earlier been considered incidental to the age of the earth and stratigraphic location. Lyell became the great protagonist of the study of fossils as a correlative of sedimentary formations. The study of fossils changed the dimensions of geology, and the altered dimensions of geology changed the nature of geological debate. Geologists and others conversant with its findings were thereafter involved in questions relating to species and their relationship to the natural environment.[9] "The first fact geology proclaims," wrote Darwin in the Sketch of 1842, "is immense number of extinct forms, and new appearances. Tertiary strata leads to belief, that forms gradually become rare and disappear and are gradually supplied by others. We see some forms now becoming rare and disappearing, we know of no sudden creation: in older periods the forms appear to come in suddenly, scene shifts: but even here . . . Genera and higher forms come on and disappear, in same way leaving a species on one or more stages below that which the form abounded."[10] The gaps in the geological record which Darwin so carefully instanced were subsequently

[9] Herbert E. Gregory, "Geology," L. L. Woodruff, ed., *The Development of the Sciences* (New Haven, 1923), pp. 207-208; Charles C. Gillispie, *Genesis and Geology* (Cambridge, Mass., 1951), pp. 73-97, 98-102; Arthur O. Lovejoy, "The Argument for Evolution Before the *Origin of Species*," Bentley Glass, Owsei Temkin, and William L. Straus, Jr., eds., *Forerunners of Darwin 1745-1859* (Baltimore, 1959), pp. 356-414; Maurice Mandelbaum, "Scientific Background of the Evolutionary Theory in Biology," Philip P. Wiener and Aaron Noland, eds., *Roots of Scientific Thought* (New York, 1957), pp. 517-538.

[10] Charles Darwin, *The Foundations of the Origin of Species, Two Essays Written in 1842 and 1844*, Francis Darwin, ed. (1909), p. 29 (italics in original).

filled or conceptually explained. Although Darwin did not have the advantage of the fossil series illustrating the development of the horse and the elephant when he wrote the *Origin*, he still felt convinced that the geological record at least established "the certain geological fact that species do somehow change."[11]

Darwin enthusiastically reported to Lyell that he regarded the chapter on embryology in the *Origin* as one of his "strongest points." Despite Darwin's confidence in his embryological sections, they made little impression on his critics. Hostile men, he complained, passed over the explanations offered of morphology, classification, and rudimentary organs as well as his explanations of embryology.[12]

Zoologists were, of course, aware of affinities and differences in nature. They were as fully aware of von Baer's law of embryonic resemblance. Why then was there no acceptable theory of transmutation? The methodological principles operative in morphology, embryology, and classification suggest a probable answer. Specialists in these fields sought a unifying principle adequate to explain natural diversity. Classification systems were based upon what were called natural affinities. Organs of different species were compared with one another to discover which organs systematically recurred. They were then pronounced typical of the class to which they were said to belong. The typical and the invariant were the goals of specialists in the classifactory branches of zoology. But a recurrence of organs, forming a clear systematic pattern, did not establish the use-value of the organ to the living form possessing it. Most naturalists assumed a necessary correlation between the two and George Romanes insisted that until the turn of the last century such a correlation was "regarded as an axiom of taxonomic science."[13] Unfortunately, however, de Candolle reported that "instead of a direct there was usually an inverse proportion between the functional and taxonomic value of a structure" (Romanes, I, 34). No connection, in other words, necessarily existed between a recurrent organ and its biological use or value. Neither De Candolle nor any other naturalist, premising special individual creations, was able to explain this anomaly. Darwin, delighting in such inconsistencies, suggested that genetic principles alone explained them. Together with Lyell and Haeckel, Darwin

[11] *Life*, III, 25.
[12] Darwin to Lyell, 25 Sept. [1859], II, 109; to same, 15 Feb. [1860], II, 285; Jane Oppenheimer, "An Embryological Enigma in the *Origin of Species*," in *Forerunners of Darwin*, pp. 292-322; Darwin to Hooker, 14 Dec. [1859], *Life*, II, 243.
[13] George John Romanes, *Darwin, and After Darwin*, 3 vols. (1892, 1895, 1897), I, 34.

employed an analogy from the study of language. The tree-like system of classification, devised by naturalists to illustrate the grand system of nature, resembled the development of language. Latin, the Romance tongues and English also represented a tree-like growth. All were related, but the relationship was explained by viewing Latin as the fossil variety and the others as possessing a genetic kinship stemming from the parent form (Romanes, I, 32-33).

Darwin's data was actually overwhelming. The *Origin of Species* was crammed with data and the evidence was expanded, refined, and elaborated in later editions of the *Origin* and in Darwin's subsequent works. The facts, often new and rare, were garnered from innumerable sources and covered virtually the entire range of natural history. Old and familiar facts were placed in startlingly novel perspective.

But the genius of the *Origin of Species* and the *Descent of Man* resides in its conceptual organization, the hypotheses of descent, natural selection, struggle, isolation, sexual selection, heredity, and kindred theories. The genius of Darwin lies in the capacity for marshalling the data, new and old, in a framework of theoretical analysis. Darwin ferreted data out of horticultural journals and breeders' records. He observed seedlings, insects, and birds with unbelievable patience. Monographs of research gave him what he called a harvest of facts and he extracted additional facts and additional ideas from his friends and coworkers. He discovered data as the result of his experiments with animals, with orchids, and with climbing plants; with volcanoes, coral reefs, and barnacles. Darwin's data were varied and magnificent, but the validity of the evidence for the Darwinian doctrine of descent by modification rested on the validity of the Darwinian hypothesis of development. And the validity of the hypothesis of evolution depended on the validity of the special hypotheses that explained its operation. Both depended on concepts of scientific method which were in turn rooted in a logic justifying its principles and its philosophy. Darwin's greatest contribution was to weld these elements together. He merged the evidence, the hypotheses, and the method in a grand analytical synthesis. As a consequence he legitimized the methodology by which the evidence was clarified and by which the hypotheses were validated.

While the evidence, to use a favorite Darwinian word, was truly "staggering," the hypothesis of natural selection was more important than the data. The hypothesis of natural selection, crucial to the establishment of the concept of evolution, is historically less vital than the mode of its establishment. It is the method which constitutes Darwin's claim to enduring greatness. Darwin's greatness rests on his total

contribution, but the refinement of the evidence of transmutation taken alone is no more than a detail in the history of science. The expansion of the method of science exemplified by Darwin in establishing the doctrine of evolution constitutes an epoch in the history of science and a landmark in the history of thought.

Whether old or new, Darwin's evidence concentrated on difficulties, difficulties hard to explain on any current biological theory. How could conventional arguments receive so wide an acceptance when they failed to eliminate stark exceptions or to explain away incongruities with other bodies of fact? Genetic derivation, he repeatedly urged, at least offered a plausible explanation. Evolution by descent, moreover, provided an hypothesis which explained many apparently diverse facts and offered a unifying interpretation of the variety and alternations of living forms. Darwin did not generally encounter opposition to his specific data; it was his interpretation of the data which was suspect. The facts of geology were as rarely in dispute as the facts of embryology. No one doubted the existence of rudimentary organs, but Darwin's explanation of them was frequently spurned. That different plants and animals were distributed over the earth was an elementary principle; but Darwin's theories of geographical distribution were hardly elementary and were not universally accepted as principles. Darwin's hypotheses were criticized because Darwin's method and its logic defied regnant philosophical views.

Darwin's exposition of classification as of embryology and taxonomy was a calculated refutation of contemporary postulates.[14] He not only set forth the evidence in evolutionary categories but he also challenged accepted maxims of interpretation and method. When discussing the natural system he remarked that "scarcely two naturalists will give the same answer" when seeking to define it. Charles Darwin was invariably temperate and polite, but his queries and comments were not on that account less devastating: "How it comes, that certain facts of the structure, by which the habits and functions of the species are settled, are of no use in classification, whilst, other parts, formed at the same time, are of the greatest, it would be difficult to say, on the theory of separate creations" (*Foundations* [1844], p. 199).

To a Richard Owen or a Louis Agassiz it must have seemed that Darwin was deliberately irritating. He was forever accenting the nega-

[14] Darwin, *Foundations* [1844], pp. 198, 199; Romanes, I, 24 ff.

tive, probing into difficulties that had long agonized his adversaries. He was purposely exposing the weaknesses of the opposition case. That there was no other way of making his own was beside the point. And Darwin was bent on making it. Certain naturalists, he commented, "believe that the degree of affinity on the natural system depends on the degrees of resemblance in organs more or less physiologically important for the preservation of life" (*Foundations* [1844], p. 200). To discover a scale of importance was unquestionably difficult. Nevertheless, the general proposition, said Darwin, "must be rejected as false; though it may be partially true." The reasons for so blunt a statement were not abstruse. Classification as interpreted by Owen or Agassiz did not work in Darwinian terms. Such systems of classification failed to work because they bore little resemblance to the conditions of life in which the organs actually functioned. Darwin was not seeking a universal plan of nature; he was seeking specific causal explanations, explanations of functioning organs in organic life and in a series of changing environments. Regardless of the particular Darwinian argument, the contention was always the same: what was the explanation of specific change? Regardless of the particular Darwinian contention, men were asked to see familiar data in new relations and the new relations were conceptual, constructs based upon methodological principles which many felt were invalid and inappropriate.

Darwin's search took him to the most unexpected places and induced him to ask all manner of unorthodox questions. "If you knew," he wrote Hooker, "some of the experiments (if they may be so called) which I am trying, you would have a good right to sneer, for they are so *absurd* even in *my* opinion that I dare not tell you."[15] Darwin's efforts to extract fresh meanings from established convictions taught him to suspect definitive solutions and to abjure the ideal of finality. He urged Hooker, who slashed at difficulties with a candor only friendship permitted, to do his worst, but he admonished him to remember that "you cannot have thought so freely on the subject as I have."[16] In making this point Darwin made an essential point about himself and about scientific method. Darwin's free-ranging explorations disposed him to regard all questions as open, to accept no conclusions drained from observed facts as beyond further extension by hypothesis. But it was just this tendency which invited dissent, for such queries as Darwin asked were exactly the

[15] Darwin to Hooker, 14 Apr. [1855], *Life*, II, 55 (italics in original).
[16] Darwin to Hooker, 12 Oct. [1858], *Life*, II, 138, and [Oct. 1856], p. 85.

queries which had been definitely settled. Yet Darwin was simply exemplifying another facet of scholarship: the determination to reject provisionally all prescribed formulations while examining competing formulations. The principle of exclusion possesses the advantage of apparent finality but it is bought at the price of ruling out probabilities. "This is experiment after my own heart," Darwin once wrote illustrating the principle, "with chances 1000 to 1 against its success."[17]

Darwin was rarely inhibited by epistemological reservations. Darwin accepted the facts of nature as provisionally given; he did not begin his inquiry with the problems of knowledge. He was in no sense an aggressive empiricist; he simply was not concerned with metaphysics.[18] If variations were accepted as given for purposes of further analysis, then subsequent changes or further variations occurred as the result of natural causes, the interactivities of the total environments. The word "origin" was never used in its sense of "beginning"; it always implied changes in the development of life-forms already in existence.

Criticisms levelled at Darwin's theory of variation have confounded his own conception of it. Whatever inferences are latent in the text of the *Origin* and other Darwin writings, Darwin's own thinking was unambiguous. "I imagine," he once wrote to Hooker, successfully restraining his irritation, "that you look at variability as some necessary contingency" within the organism, "that there is some necessary tendency in the variability to go on diverging in character or degree." "I do not agree," he said simply.[19] "The formation of a strong variety or species I look at as almost wholly due to the selection of what may be incorrectly called *chance* variations or variability." "No doubt . . . variability is governed by laws, some of which I am endeavouring very obscurely to trace."[20] They were incorrectly called chance variations because factors as yet unknown doubtless explained them, but they were none the less random in the sense that they were unwilled and unplanned. Wallace, as so often happened, was even clearer. Variation, after all, was no more than *"the absence of identity,"*[21] and required no additional elaboration. To Darwin he wrote: "variations of every kind

[17] Darwin to Hooker [1855], *Life*, II, 57.
[18] *Life*, I, 69. But see Robert E. Fitch, "Charles Darwin: Science and the Saintly Sentiments," *Columbia University Forum*, II (1959), 7-12.
[19] Darwin to Hooker, 11 May [1859], *Life*, II, 158; Darwin to Lyell, 25 Oct. [1859], pp. 176-177.
[20] Darwin to Hooker, 23 Nov. [1856], *Life*, II, 87.
[21] Alfred Russel Wallace, "The Origin of Species and Genera," *Studies Scientific and Social* (1900), I, 302 (italics in original).

are always occurring in every part of every species, and . . . favourable variations are always ready when wanted."[22]

Darwin was employing the objective method of science. He was seeking to define "natural laws," by which he meant the identification of sequences occurring in nature. These sequences expressed relations among phenomena and offered explanations of their behavior without reference to ultimate causes exterior to the phenomena under review. Darwin did not seek to explain the quality or essence of a prehistoric barnacle or to assign a primary cause for the existence of the varieties of Alpine flora. He observed, compared, and measured varieties in historical and contemporary contexts, and sought to discover connections within the economy of the individual organism as well as within the economy of nature. Darwin did not seek for final causes of gill, fin, wing, or bone. He always asked what these organs did, how they changed, what purposes they served under one set of conditions or under another.

Darwin held universal theories in abeyance. "I find," he wrote the cosmic philosopher, John Fiske, "that my mind is so fixed by the inductive method, that I cannot appreciate deductive reasoning." He did not deny Fiske's right to construct a cosmic philosophy, he simply explained why he was unable to do so. "I must begin with a good body of facts, and not from principle (in which I always suspect some fallacy), and then as much deduction as you please."[23] Darwin understood by deduction not only reasoning from the general to the particular, from a principle or law to a particular fact, but deducing facts from principles which were fixed beyond investigation. "I have no faith in anything," he once remarked, "short of actual measurement and the Rule of Three."[24]

Darwin stuck to his scientific guns and often fired broadsides at his detractors. "It is mere rubbish," he exploded in a letter to Hooker, "thinking at present of the origin of life; one might as well think of the origin of matter."[25] "What," he asked in the 1842 Sketch, "would the Astronomer say to the doctrine that the planets moved [not] according to the laws of gravitation, but from the Creator having willed each separate planet to move in its particular orbit?" (*Foundations*, p. 22). Darwin was unequivocal. In an early criticism of the manuscript that was to become the *Origin*, Lyell unravelled his reservations. Darwin grate-

[22] Wallace to Darwin, 2 July [1866], *More Letters*, I, 270; Darwin, *Foundations* [1842], pp. 1-2.
[23] Darwin to Fiske, 8 Dec. [1874], *Life*, III, 193-194.
[24] *Life*, II, 51 [1855].
[25] Darwin to Hooker [29 Mar. 1863], *Life*, III, 18.

fully acknowledged them, but he had already outdistanced his master in refining a philosophy of science. Lyell propounded what for him was a fundamental question: *"must you not assume a primordial creative power which does not act with uniformity, or how else could man supervene?"*[26] To this question Darwin replied: "under present knowledge [we must assume] the creation of one or a few new forms in the same manner as philosophers assume a power by attraction without any explanation." The remainder of Darwin's answer is an excellent statement of his own position and one of the best statements of the philosophy of scientific method.

> I entirely reject, as in my judgment quite unnecessary, any subsequent addition of "new powers and attributes and forces": or of any "principle of improvement", except in so far as every character which is naturally selected or preserved is in some way an advantage or improvement, otherwise it would not have been selected. If I were convinced that I required such additions to the theory of natural selection, I would reject it as rubbish, but I have firm faith in it, as I cannot believe, that if false, it would explain so many whole classes of facts, which, if I am in my senses, it seems to explain. . . . I would give absolutely nothing for the theory of Natural Selection, if it require miraculous additions at any one stage of descent. I think Embryology, Homology, Classification, &c., &c., show us that all vertebrata have descended from one parent; how that parent appeared we know not. If you admit in ever so little a degree . . . you will find it difficult to say: thus far the explanation holds good, but no further; here we must call in "the addition of new creative forces." I think you will be driven to reject all or admit all: I fear by your letter it will be the former alternative; and in that case I shall feel sure it is my fault, and not the theory's fault, and this will certainly comfort me.[27]

The Darwinian theory of evolution itself was Darwin's most impressive witness. Darwinian evolution was an hypothesis. As such it was a theoretical instrument devised to organize and test the data. There were no incontrovertible facts attesting that one variety had been transmuted into another. There was no concrete evidence to establish that random variations, basic to the operation of natural selection, existed in nature and had always so existed. Darwin could not even demonstrate concretely the process by which altered physical characters in a given organism were transmitted to succeeding generations. General critics flayed the theory as "mere hypothesis," hypothesis in the sense of unsubstantial speculation. That Darwin's theory was "vain" was underscored by the popular notion of the role of the scientist as a collector of facts. When the facts were collected and organized, they then spoke for themselves. Scholars – philosophers, scientists, and theologians – re-

[26] Darwin to Lyell, 11 Oct. [1859], *Life*, II, 210 (italics in original).
[27] *Life*, II, 210-211.

jected Darwin's theory on more sophisticated grounds. For them the hypothesis violated the logic of hypothesis. It constituted a perversion of the canons of induction, for assumptions, not facts, were offered as the ground of its credibility. The learned William Whewell's critique of Hume applied with equal force to Darwin: "Our inference from Hume's observations is, not the truth of his conclusions, but the falsehood of his premises: not that, therefore, we can know nothing of natural connexion, but that, therefore, we have some other source of knowledge than experience." The philosophy of induction derived from the philosophy of causation. "Cause is to be conceived as some abstract quality, power, or efficacy, by which change is produced; a quality not identical with the events, but disclosed by means of them."[28]

With Lyell, constantly concerned about the ultimate, Darwin conducted a running debate, a debate Darwin called "our quasi-theological controversy about natural selection." "Do you consider," he asked Lyell, "the successive variations in the size of the crop of a Pouter Pigeon, which man has accumulated to please his caprice, . . . due to the creative and sustaining powers of Brahma?"[29] Darwin could not accept this inference. Since he could not imagine that Lyell would think otherwise, he wondered how any logical difference could be established between selection produced by the breeder under domestication and the result of selection in a state of nature.

If Darwin's debate with Lyell was quasi-theological, his debate with Asa Gray was frankly theological. He was, he confessed, "in an utterly hopeless muddle." Yet he could not endorse Gray's resolute faith in design: "I cannot look at each separate thing as the result of Design."[30] However, he was not sufficiently muddled to be without a point of view. Questions of ultimate purpose, he told Lyell, are "beyond the human intellect, like 'predestination' and 'free will,' or the 'origin of evil'."[31] Darwin entertained wistful thoughts about ultimate meanings, but he never permitted such thoughts to interfere with his operations. He did not know, to be sure, where he was coming out, but he struggled conscientiously to suspend judgment about the purpose of the

[28] William Whewell, *The Philosophy of the Inductive Sciences* (1840), I, 72; Alvar Ellegård, "Darwin's Theory and Nineteenth Century Philosophy of Science," in *Roots of Scientific Thought*, pp. 537-568; C. J. Ducasse, "Whewell's Philosophy of Scientific Discovery," *Philosophical Review*, LX (1951), 56-69, 213-234; E. W. Strong, "William Whewell and John Stuart Mill: Their Controversy About Scientific Knowledge," *Journal of the History of Ideas*, XVI (1955), 209-231.
[29] Darwin to Lyell, Apr. [1860], *Life*, II, 303-304.
[30] Darwin to Gray, 26 Nov. 1860, *Life*, II, 353.
[31] Darwin to Lyell, 25 Apr. [1860], *Life*, II, 304.

universe while he was engaged in the scientific examination of its terrestrial parts. "I am inclined," he admitted to Gray, certain his admission would not please him, "to look at everything as resulting from designed laws, with the details, whether good or bad, left to the working out of what we call chance. Not that this notion *at all* satisfies me. I feel most deeply that the whole subject is too profound for the human intellect. A dog might as well speculate on the mind of a Newton. Let each man hope and believe what he can."[32]

Darwin's attitude toward cosmic purpose resembles the attitude of Chauncey Wright. "When it was objected to him," William James recalled, "that there must be some principle of oneness in the diversity of phenomena – some *glue* to hold them together . . . he would reply that there is no need of a glue to join things unless we apprehend some reason why they should fall asunder. Phenomena *are* grouped – more we cannot say of them."[33]

Darwin and Chauncey Wright were in general accord. Both were advocates of the neutrality of science. "True science," wrote the American logician, "should approach . . . questions, avoiding . . . the terms which have attached to them *good* and *bad* meanings in place of scientific distinctness, – terms which have a moral connotation as well as a scientific one . . . Words have 'reputations' as well as other authorities, and there is a tyranny in their reputations even more fatal to freedom of thought. True science deals with nothing but questions of facts – and in terms, if possible, which shall not determine beforehand how we ought to feel about the facts; for this is one of the most certain and fatal means of corrupting evidence."[34]

Darwin granted special weight to no category of probable meanings. Neither Cause, Being, Life, or Design was accorded any special prior significance. Darwin accounted life a given of nature. If it had larger meanings, those meanings were yet to be determined. Ultimate meanings were not within the scope of science. Wright, following Kant, distinguished between science and ethics, and Darwin in practice agreed with both. "If the facts are determined, and, as far as may be, free from moral biases, then practical science comes in to determine what, in view of the facts, our feelings and rules of conduct ought to be; but practical science has no inherent postulates any more than speculative

[32] Darwin to Gray, 22 May [1860], *Life*, II, 310-311, 312.
[33] William James, "Chauncey Wright," *Nation*, XXI (1875), 194.
[34] Cited from the Wright papers by Philip P. Wiener, *Evolution and the Founders of Pragmatism* (Cambridge, Mass., 1949), pp. 44-45.

science. Its ultimate grounds are the particular goods or ends of human life."[35]

A vital aspect of the Darwinian revolution consists in Darwin's contribution to the method and philosophy of science. The nature of the Darwinian revolution is the revolution in man's conception of nature. Copernicus brought astronomy within the domain of science. Darwin brought zoology within the domain of science. Darwin made the biological sciences objective, experimental, phenomenal, and empirical. As Darwin viewed the world of Reality, Being possessed no antecedent priority. Being, if Being there was, was to be found in the process of natural exploration. Darwin contributed to reverse the order of Being and process. Being and permanence were conceived, not as ultimate coordinates of temporal change within an antecedent transcendental system. They were conceived as logical coordinates emergent in the study of natural parts. Historically, the question is not whether Parmenides was wrong and Darwin and his successors right. Significant is the fact that Darwin separated the study of process from the concept of Being. The impact of Darwin is the impact of scientific method on the life sciences and on the sciences of human life. The laws of moral "harmony," Chauncey Wright is reported to have said, "are of a wholly different order, *different in meaning* . . . neither contradictory to nor in conformity with those of the scientific cosmos."[36]

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[35] Wiener, pp. 44-45; A. D. Lindsay, *Kant* (London, 1934), pp. 163-164.
[36] Wiener, p. 36 (italics in original); Charles C. Gillispie, "Lamarck and Darwin in the History of Science," in *Forerunners of Darwin*, pp. 265-291.

Morse Peckham

DARWINISM AND DARWINISTICISM

EVERYONE KNOWS THAT THE IMPACT of the *Origin of Species* was immense and that it has had a profound influence upon the literature of England and of the West ever since that late November day in 1859. But when one tries to tally up the writers affected and to list the books and make an inventory of passages showing Darwinian influence and Darwinian assumptions in novels, poems, and essays, a fog seems to arise in one's mind, through which are discernible twinkles of what may or may not be bits of genuine Darwinism.

Indeed, here is the first problem. What is Darwinism? Or at least, what is the Darwinism found in the *Origin*, for that alone is the Darwinism with which I am here concerned. The name of Darwin has magnetized to itself a thousand bits and pieces of ideas which are certainly not be found in the book itself, and some of which Darwin, had he been able to understand them, would certainly have repudiated. For example, it has been said a million times and will be said a million times more that for Darwin competition between species and members of spe-

cies is the only mechanism of directive and progressive evolution. Thus he has been adulated for having revealed that in capitalism and its related and derived values was to be found the natural system of social and economic organization which assures the progress of man; and execrated for having led men to disbelieve the seemingly obvious truth that man's proper, natural, and normal mode of behavior is cooperation, harmony, and love. Again, it is always being rediscovered that in natural selection there are cooperative as well as competitive mechanisms at work. Alternatively, it is frequently stated that Darwin failed to perceive the element of cooperation because he was himself the product of a laissez-faire society: Marxists are particularly grand and imposing on the subject.

In this one example are to be found the typical confusions observable in many discussions of Darwinism carried on by non-scientists and even by scientists when they are not scientizing. First, there is by the very use of the terms an introduction of values into a descriptive construct; or the misinterpretation of descriptive terms by ascribing to the words a moral significance and to the author a moral intention. The roots of this error are to be found in the ancient exhortation that Nature should be our basic model for Right Behavior. Not surprisingly it is constantly assumed that the *Origin* rests on moral assumptions: that a value statement may be verified in the same way that an empiricial or predictive statement is verified is an attitude that only a small fraction of human beings have yet outgrown, and that in only a small part of their behavior. The difficulty arose because Darwin did not have the word "ecology." He was in fact an ecological thinker, and in ecology words like "competition" and "cooperation" are too inexact, too value-weighted, too metaphorical, and too anthropomorphic to be used at all.

Second, scientific statements are continuously subjected not only to moral interpretations but also, and more subtly, to metaphysical interpretations. Hence, the unconscious ascription to the *Origin* of a metaphysic. Darwin is said to have discovered the Law of Evolution, according to which the universe is characterized by a steady growth in richness and complexity and excellence. Now Spencer formulated a Law of Evolution, but there is no such law in the *Origin*. In fact, in the fourth edition there is a brief but profoundly important passage at the beginning of Chapter Four in which Darwin specifically disclaims any knowledge of and any statements about Laws of Nature, which he clearly labels mental conveniences, or constructs. He is a scientist, not a moralist and not a metaphysician, and he knows it. Unfortunately, most humanists then and now have little notion of what a scientist is and does. They are in-

terested in metaphysics and morals, and when a science seems to have a metaphysical and moral implication, or when a scientist assumes another role and makes metaphysical and moral statements, then only do they evince an interest and subject themselves to what they think are scientific influences.*

It will be apparent from what I have said so far that the problem of the impact of the *Origin* upon the culture of the last one hundred years is a complex one. It involves, above all, the question, what is the difference between "Darwinian" and "Darwinistic" (that is, between those propositions and implied assumptions which may be properly ascribed to a source in the *Origin*, and those propositions and derived assumptions which are not properly so ascribed). I shall attempt to clear the way for future studies of the *Origin* and literature, using "literature" generally and loosely.

The question to be answered, then, is, what was the Darwinian and Darwinistic impact of the *Origin* upon Western culture? But even this is much too vague. How is the culture of the past available and what do we mean by "culture"? It is said that historical investigation is concerned with the events of the past. But of course past events are not observable, and so we are left with the question as to what in fact the historian observes. The problem may be put somewhat crudely, though I think not naïvely, tackled by observing what the historian actually does. He is engaged in the manipulation (with all that the word implies) of documents and artifacts. These are his data, not past events. The empirical referent of his "History," that is, his construct which purports to refer to the events of the past, or "history," is his own operations with those documents and artifacts which he assumes to have had an existence prior to the moment he began examining them. His History is a linguistic construct, characterized by what he hopes is at least an intuitive consistence, designed to justify his work. Since internal inconsistencies are always discoverable in his construct, and since his collection of docu-

* The question of the influence of the *Origin* hangs on not merely, what is Darwinism? or, what is the Darwinism of the *Origin*? but also on what is the Darwinism to be found in which edition of the *Origin*? In my recent variorum text of the work are to be found between fifteen and twenty thousand individual variants, or well over seven thousand sentence variants, a sentence variant ranging from the exclusion or addition of a sentence to a minor and apparently non-semantic change in punctuation. The majority of these are probably of not very great importance, but there are thousands of sentence variants of crucial significance. The collation of these variants reveals how difficult the question of Darwinian influence really is, but at least in the future it will be possible to find out if Darwin really made a statement ascribed to him.

ments and artifacts is never complete, nor its degree of completeness ever known, History, like any science, is characterized by instability. (Like physics, it is always being reconstructed.) Thus the process of historiography is the consequence of a continuous interaction, manipulated by historians, between construct and data.

One particular problem remains:* what is the model of the historian's construct. Such models certainly are now, and perhaps always will be, intuitive. At least I know of no mathematical or logical model for a historical narrative. But though models for battles, parliamentary debates, and assassinations are probably pretty reliable, those for cultural history are probably not. In cultural history we are involved with matters of extraordinary difficulty and subtlety. Whereas the assassination of Lincoln was eminently observable at one time (there were witnesses; a reasonably consistent construct may be created of his murder and death), a priest may lose his religious belief and nobody be the wiser (Pater thought he could have a successful career as an Anglican clergyman with no religious belief whatsoever, and he was probably right). The difficulty lies in the fact that the locus of cultural history is covert behavior. When we realize that there is no such entity as "culture" but only human beings doing something, or behaving covertly, and that such behavior may or may not leave traces in documents and artifacts, the general problem of the inaccessibility of the past is twice compounded; for cultural behavior is not necessarily observable even when it is going on in a human being who is right before the observer's eyes or even at times, when it is going on inside the observer. When we talk about "culture" in the old-fashioned way, or "high-level culture" in the modern way, we are referring somewhat vaguely to two kinds of data, first, recorded verbal behavior, sufficiently complex and highly-valued to be called literature, philosophy, or science, and recorded sign-systems of a parallel complexity which we call the visual and auditory

* Another question is more easily answered and is less important to the problem of this essay, for the historian derives from his construct certain propositions which purport to predict where certain documents and artifacts may be found and what will characterize them when they are found. Thus a construct led me once to predict that certain unidentified watercolor illustrations to Milton would be found to have been prepared between 1790 and 1810 for copper-engravings to illustrate an edition of *Paradise Lost*. An examination of such editions led to the discovery of one with such engravings and with the name of the artist. An intuitive construct of stylistic history had led me to predict that they had been done about 1800, and according to bibliographical information derived from the engravings themselves they could be dated in 1797 and 1798. Finally an intuitive model of the behavior of artists and publishers led me to conclude that the watercolors had not been done more than three or four years before the date of first publication.

arts, and, second, unrecorded and covert linguistic and aesthetic semiotic behavior. On the model of our own behavior, insofar as we can and have trained ourselves to observe it, aided by various admittedly inadequate personality or psychological theories, we try to write cultural history. Now, we know very little about cultural transmission and cultural innovation at any level, let alone complex high-level transmission and innovation. It is not surprising that when we undertake to write cultural history we finish feeling a bit baffled and inadequate.

At least, however, we can now see our basic problem somewhat more precisely. On the one hand there are the Darwinian and Darwinistic documents, on the other, documents and artifacts which show Darwinian and Darwinistic influences, and in between, causally, we hope or assume for constructive purposes, connecting the two categories, a doubly inaccessible process of cultural history of which we wish to make a construct. But, alas! each part of the problem consists of a set of variables. For instance, a single Darwinistic sentence might have had a profoundly revolutionary impact upon the covert behavior of an individual, with the consequence that he wrote a single poem profoundly different from anything he had written before. (Did this happen in the case of Swinburne's "Hertha"?) Or a reading of the entire *Origin* might have had a slight impact which resulted, for the moment, at least, in a fairly brief document. (Was this the case of Kingsley's famous letter which Darwin was so happy to quote in later editions in order to avert the theological lightning?) Or a very thorough reading of Darwinian and Darwinistic material might so confirm the already existing attitudes of an individual that he perceived nothing innovative. (Was this the case with Browning, whose post-*Origin* work indicates that he not only read Darwin but read him with such extreme care that frequent re-reading is implied?) To analyze this range of variables is the next task.

I have already given one example of a fairly notorious confusion between Darwinism and Darwinisticism. I wish now to treat this problem more extensively, and particularly to indicate how the two came to be confused. I shall begin with one of the biggest questions, evolution itself. Evolution may be considered as a fairly straightforward metaphysical theory with a long history which was not so much confirmed by the theory of natural selection as embarrassed by it. The difference between the two is indicated by the fact that Darwin himself did not use the word until the fifth edition of the *Origin* (1869), and then he appears to have used it with some hesitation, almost as if he did not quite know what he was talking about. And no wonder. I have called it a

metaphysical term, because it purports to make certain descriptive propositions about the structure of the universe. One way to indicate its significance is to employ a metaphysical distinction between appearance and reality and their relation to permanence and change. According to a non-evolutionary metaphysic the changes which we observe are only apparent; the reality of things lies in their permanence. The exalted status of Platonism in the Western tradition, both in theology and out of it, was one of the principal reinforcements of a non-evolutionary metaphysic. In his recent intellectual autobiography, *My Philosophical Development*, Bertrand Russell has related how his early interest in mathematics was stimulated by a desire to penetrate a world of ideal permanence and perfection and he admits that he still has a certain regret that Wittgenstein persuaded him that mathematics and logic are tautological and do not refer to any such ideal realm. But Yeats, Russell's contemporary, and the hard-headed and profoundly anti-metaphysical Nietzsche toyed with eternal recurrence, though how seriously they wished us to take them presents an apparently unresolved problem for their commentators. All of these men were post-*Origin*, and they all evince the distress which resulted from the apparent confirmation of metaphysical evolutionism by the scientific construct of natural selection.

For according to a metaphysic of evolution, the appearance lies in permanence, the reality lies in change. The permanence of things, whether houses or social institutions, is only a function of the fact that they change more slowly than we the observers do. But the difference between evolution and a Heraclitean doctrine that all things are a flowing lies in the proposition that evolution is directive. As I have indicated above, the claim that natural selection works in an orthogenetic or teleological fashion is an application to Darwinism of the already existent and widespread notion that the laws of nature direct the course of events to move towards a higher, purer, better state of affairs.[1] I have encountered in recent years a theology which claims that the history of the universe is the history of God ridding himself and his creation of evil, and that the purpose of man's existence is to help God in this sublime task.

Hegel is, of course, the figure whom everyone thinks of in connection with metaphysical evolution, and "One far-off, divine event/ Towards which the whole creation moves," the lines that everyone quotes. But the concept is ubiquitous in the earlier nineteenth century. It seems, at first glance, almost impossible that Browning could have

written Paracelsus' great final speech a quarter of a century before the *Origin*. In the cancelled stanzas of the "Palace of Art,"* written even earlier, Tennyson applied metaphysical evolutionism to the data of science, specifically astronomy and geology. It is implicit in Carlyle's theory of history, in which each inorganic period is less inorganic than the preceding one, and each organic period more thoroughly organized than its corresponding predecessor — an idea which seems to inform *The Idylls of the King*, the first installment of which came out in the same year as the *Origin*. Newman has both the notion of progressive revelation and, of course, of the growth of doctrine.

The two essential ideas of metaphysical evolutionism are that the world is an emergent world, with novelty continually being introduced from outside or coming into existence by the nature of the evolutionary process, and that it is an irreversible and a non-repetitive world; evolution has direction.

Even before Darwin, as Tennyson's stanzas suggest, there had been apparently scientific substantiations of the evolutionary metaphysic. Ever since 1839, J. P. Nichol, the astronomer, had been writing and re-writing a whole series of books popularizing astronomy. Over and over again, basing his conclusions on the nebular observations of the younger Herschel, he had painted a picture of a universe in the process of creation and in 1855 had used the word "evolution" to describe it. It is hardly necessary to point out how Lyell gave apparent confirmation to the notion at the geological level. Yet the evolutionary metaphysic is not necessarily or logically connected with scientific constructs about the biologic past. The term, "the development hypothesis," which was employed throughout the century until in the 1860's "evolution" took its place, is considerably more descriptive and relatively free from the metaphysical implications of "evolution."

To understand what happened, it is necessary to keep the distinction between the two quite clear. The development hypothesis was a scientific hypothesis concerned with the problem of how biological species originate. Are they specially created by God, or do they somehow

* Hither, when all the deep unsounded skies
Shuddered with silent stars, she clomb,
And as with optic glasses her keen eyes
Pierced through the mystic dome.

Regions of lucid matter taking forms,
Brushes of fire, hazy gleams,
Clusters and beds of worlds, and bee-like swarms
Of suns, and starry streams.

or other develop out of already existent species? As early as 1827 Lyell wrote in a private letter that he believed the latter, but since he had no explanation of how it could have happened, he had to use, in his great *Geology*, the theory of special creation. The scientists were of course perfectly aware of the implications, for contemporary theologians, of denying the theory of special creation, and they had no desire to get into trouble unless for a good scientific cause. Hence their lack of interest in Chambers' *Vestiges of Creation*, and even annoyance with it, for it aroused theological fury to no good scientific purpose, simply substituting one metaphysic for another. What was above all necessary was a scientific explanation of what a great many scientists were quite prepared to accept, the origin of species from pre-existing species.

The crux of the matter is revealed by comparing Spencer's essays and books with the *Origin*. In his introduction to the *General Principles* Spencer complained that Darwin had received credit for establishing biologic evolution, although, in fact, he had already revealed the truth several years before. And of course a great many people, particularly in the United States, believed him. Yet Spencer has all but disappeared from contemporary culture while the current scientific theory of biological evolution still has a firm, if partial, place for Darwin's theory of natural selection and admits and insists that the *Origin* was the foundation for all subsequent work. Scientifically, Spencer's theories on the subject were of no importance; they were entirely metaphysical, more complicated than Chambers' but not less naive. Spencer uttered a Law of Evolution; Darwin proposed a theory of the origin of species from pre-existent species.

In the later chapters of his *Great Chain of Being*, A. O. Lovejoy has shown how metaphysical evolutionism emerged from a kind of static, Platonic thinking which had dominated the Western mind for over two millenia. In my "Towards a Theory of Romanticism" (*PMLA*, LXVI [1951], 5-23), I attempted to show some of the cultural forces responsible for that shift in metaphysics. Using the argument of that essay, and developing it further, I should like here to suggest how the evolutionary metaphysic, although it had an unimportant previous existence, was accepted and developed in the early nineteenth century.

To use the terminology of that article, the "Negative Romantic" was left without any metaphysic and without any theoretic basis for morality. In his isolated state he was thrown back upon himself. But since it is extremely difficult to maintain a total spiritual death for a very long period, some of the Negative Romantics simply returned to former beliefs. But others turned to a new metaphysic, which in its many forms

can be generally subsumed under the rubric of Evolutionism as I have defined it above. In this process of recovery there were usually two stages. First came the stripping bare of the self, the discovery of the ego as the basic datum of experience. That revelation is one of the principle themes of works as various as the *Prelude*, the *Ancient Mariner*, *Sartor Resartus*, the paintings of Constable after 1808, of Caspar David Friedrich beginning about the same time, and of Turner after 1827. It is the subject of Berlioz's *Harold in Italy* and of Schumann's piano cycles of the 1830's. It is the theme of *The Red and the Black* and even, faintly enough, *Rob Roy*. This discovery of a basic datum, however, was not sufficient. It had to be connected with the rest of the world; the problem of how to get from the self to the world remained to be solved. For its solution there was only the data of the self and its experiences, of which now the most important event was the recovery or rebirth consequent upon the discovery of the self. If the self is the model of the world, and if the process of the self in moving from negation to affirmation is its essential character, it then followed that the character of the world is not merely process but process directed toward a goal.

The weakness of this position was precisely in assuming that the self was a model of the world; but that assumption could be and was easily overlooked, for Western culture was filled with all sorts of notions which would provide the bridge between self and world. Hence the mental construct by which the processes of the personality were organized under the terms "self" or "ego" or "Ich" or "I" was projected upon the experienced world, and as Coleridge put it, the act was substituted for the fact. The tremendous emotional force behind this projection made it easy to find its confirmation, first, in already existent evolutionary metaphysics, and second, in apparent scientific confirmations of evolutionary metaphysics. The total development is perhaps most clearly outlined in *Paracelsus*, which reveals Browning as a man who grasped the romantic culture of his century with astonishing profundity. Hence it is no accident that the conclusion of the poem* sounds like an anticipation of Darwin — to one who has not really understood the *Origin*.

* Paracelsus describes the formation of the surface of the earth, then the coming of vegetation, insects, birds, land-animals, and he continues:

> Thus he [God] dwells in all,
> From life's minute beginnings, up at last
> To man — the consummation of this scheme
> Of being, the completion of this sphere

In 1859 a metaphysic of goal-directed organic growth was dominant in the higher levels of Western culture. Canon Raven has pointed out that the agitation produced by Darwin can not be understood without reference to the *Essays and Reviews*, which appeared only a few months after, or to Colenso's work, which appeared in 1862. The academic difficulties of Max Müller and of Benjamin Jowett are equally inseparable from the total situation. Today it is somewhat difficult to understand why all of these books and events should have been interconnected in the covert culture of the day. But a reading of *Essays and Reviews* shows the deep penetration of metaphysical evolutionism into the minds of its authors. That was their offense: they were addressing a world of a lower cultural level which still lived by a static metaphysic. I have referred to Newman's speculation about the possibility of applying his ideas of development to the biological world, and he has been praised for his profundity in thus anticipating and understanding Darwin. But the praise has come from individuals who did not themselves understand Darwin, for in fact the *Origin* was an embarrassment to the metaphysical evolutionists.

The evolutionists were already used to having their metaphysic apparently confirmed by scientific developments. Lyell and Herschel and Nichol seemed to support them, and they took the *Vestiges of Creation* to be a scientific work. Consequently, there is no indication that the *Origin* disturbed Tennyson, for example, or Newman, or George Eliot. "Hertha," again, is a good example. Written in the latter half of the 1860's, it appears to be a perfect instance of the impact of Darwin. Yet it can just as well be thought of as a humanistic and anti-religious interpretation of the concluding speech of *Paracelsus*. It could have been written had the *Origin* never been published. That it was written as a consequence of the *Origin* seems highly probable, but as a consequence of a misunderstanding of the *Origin*. For the biologic world that Darwin revealed, if you do not read him with the assumptions of metaphysical evolutionism as instruments for understanding the book, is a world totally lacking in the organized and teleological process characteristic of

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Of life: whose attributes had here and there
Been scattered o'er the visible world before,
Asking to be combined, dim fragments meant
To be united in some wondrous whole,
Imperfect qualities throughout creation,
Suggesting some one creature yet to make,
Some point where all those scattered rays should meet
Convergent in the faculties of man.

evolutionary metaphysics. New species come into existence by a process which can only be described as accidental. If a species has a range of variations among its individuals such that when the environment of the species changes, hitherto non-adaptive variations are selected as means of survival, a new species will in the course of time emerge, provided that the change in environment is not so great as to cause the total extinction of the species and provided that the population of the species is sufficient to maintain itself during the period of the development of the new species out of the old. No organism, Darwin said, is as perfectly adapted as it might be. That is, it includes within its population instincts and organs which are not instrumental to its environmental adaptation, although in its ancestry at some time or other such organs and instincts were adaptive. (To be sure, Darwin was not very certain about this and proposed other reasons, particularly morphology, for non-adaptive organs.) Further, there are numerous instances not only of extinction but of total or partial regression, or regression at some stage of an organism's life-cycle. Nor is there any reason to believe that natural selection operates in a morally or metaphysically progressive fashion. Indeed, from the *Origin* it is very easy to conclude that the more complex an organism the less its chances to survive. It is impossible to find in the *Origin* a basis in the biological world for any kind of orthogenesis or goal-directed process. Consequently it has been misread or simply not read at all, though discussed by all varieties of metaphysical evolutionists from Newman to Gerald Heard and current Catholic theologians.

Thus the grand thesis of metaphysical evolutionism — from simple to complex means from good to better, infinitely or finitely, as your metaphysical taste determines — not only received no support from the *Origin* but, if the book were properly understood and if the individual involved felt that a metaphysic should and could have scientific support, was positively demolished. Hence the curious spectacle, to be found so often, of orthodox fundamentalist Christians and anti-Christian or quasi-Christian metaphysical evolutionists ranged side by side in opposition to Darwin. And hence the equally curious spectacle of other metaphysical and Christian evolutionists swallowing the book without even a catch in the throat.

But the book presented an even deeper problem of which very few were apparently aware, although it was implied in the early editions and was clearly spelled out in the fourth. In the middle of the century most scientists characteristically conceived their task as discovering the laws of nature. Newton's Law of Gravity was held to be unrepeal-

able, and for a hundred and fifty years Law had been gradually replacing Providence at the higher cultural levels. To be sure there were exceptions. Newman preferred to believe that the weather was controlled by angels, and he has several very pretty passages on the subject. But for the most part advanced thinkers felt like Nichol, who in 1839 hailed the advent of the comprehension of Nature through discoverable Laws and rejoiced at the disappearance of the capricious ways of Providence. He felt it added greatly to the dignity of God to think that He governed the world through Laws and not through unpredictable wilfullness. It is hard not to see the influence here of political liberalism. Or perhaps it is the other way around. Certainly the two seem to be connected, for the idea of the subjection of the Crown to the rational laws of elected representatives, the progress away from capricious tyranny, is remarkably like Nichol's conception. An Enlightenment idea, it was a basic ingredient of most Romantic metaphysics and fundamental to evolutionary metaphysics. The metaphor unconsciously used seems too transparent to be missed, and yet it was missed. The notion was that just as it is possible to study the political behavior of a people and hence deduce its laws, so it is possible to study the behavior of nature and arrive at the laws which govern it. It was a notion particularly appealing to Englishmen, who, because of the peculiar and highly admired nature of the English Constitution, were in part governed by written laws and in part by laws not spelled out in a written constitution in the style of the infidel Bentham and the misguided Americans but implicit in the political structure of the nation. In Nature the laws are there, they are immanent in the natural world, and it is the will of God that in our enlightened progress we should discover them. Constant reference to such economic ideas as the Law of Supply and Demand continually reinforced such concepts; and it is typical that Ruskin's aesthetic and socio-economic writings are filled with Laws of This and Laws of That.

The *Origin* did not fit into this conception at all. Not only was an apparently accidental world revealed, but even if one insisted upon discovering an order in the apparent chaos of the biological world, the incredible intricacy of ecological relations was such that Darwin himself felt that a full comprehension was beyond him or any human being. When biology can be studied mathematically – and biologists are making progress in that direction, particularly in genetics – perhaps a few people will comprehend the biologic world, but it is too much for mathematically unorganized descriptive language.

Thus at the time there were frequent complaints that Darwin did not really reveal the Laws of Nature. The culture was still penetrated

by the Baconian notion that the Laws of Nature are not only immanent, because they had been put there by God, but also were few in number and essentially simple, because God had so designed them that the human mind might understand them. The culture accepted the Baconian notion that if one assembled all the data pertinent to a line of inquiry, the true relations between the separate bits of data and the laws that governed them would reveal themselves. Again we find, metaphysically, the desire to penetrate into a world of pure order.

But Darwin's notion of scientific law was empiricistic and extraordinarily modern. He implies it in his superb discussion of the term "species" and its related words. He demonstrates that to the term there is no corresponding reality or entity in the biological world. It is essential to his argument that species should not be regarded as fixed, and he disproves their fixity not so much by aligning data as by analyzing the term to demonstrate that the attempt to find distinct species in nature is necessarily fruitless, since the term is only of convenience in creating hypotheses, or, as we should say today, constructs. Further, he spelled his notion out in additions made to the opening of Chapter Four in the fourth edition. To him a scientific law was a mental convenience. The mind organized the data into meaningful structures; it did not discover the principles of organization immanent within the data. To a certain extent his public was at least intuitively aware of this position and responded to it negatively. Nevertheless for the most part the Legalists of Nature simply derived from the *Origin* further proof of their arguments. It is yet another example of how Darwinism was converted into Darwinisticism and is comparable and related to the similar absorption of the *Origin* and natural selection into metaphysical evolutionism.

There are further variables in this part of the basic question, such as adaptation, economy, and morality. Huxley, in his Romanes lecture, and Mill, earlier, in *Three Essays on Religion*, concluded that the ancient "Follow nature" as a basis for morality was in error, and that if a genuine morality were to be developed it must be on a purely human basis, indifferent to and even opposed to the workings of evolution. The economists, as we have seen, followed a different course, and with the aid of Spencer found in the *Origin* a basis for their own morality. And in the matter of adaptation, the final basis of British empirical theology, the notion of perfect adaption of organism to environment was washed away by the *Origin*. Consequently, as might have been expected, Darwinisticism in the field of moralized psychology has used Darwin as a basis for attempting to make perfect adaptation of the individual personality to its social environment into the criterion of psychological

health. There can be located still other variables, but I have attempted to go into at least two of the problems involved in the impact of the *Origin* in order to show the profound difference between Darwinism and Darwinisticism. Darwinism is a scientific theory about the origin of biological species from pre-existent species, the mechanism of that process being an extraordinarily complex ecology which can be observed only in fairly small and artificially isolated instances. It reveals a world not of accident precisely but rather one in which "accident" becomes a meaningless problem. Darwinisticism can be an evolutionary metaphysic about the nature of reality and the universe. It can be a metaphysical and simplicistic notion of natural law. It can be an economic theory, or a moral theory, or an aesthetic theory, or a psychological theory. It can be anything which claims to have support from the *Origin*, or conversely anything which claims to have really understood what Darwin inadequately and partially presented. Once one is aware of the distinction, much which is ascribed to Darwin and much which appears to be Darwinian in the cultural documents of the past one hundred years turns out not to be Darwinian at all but Darwinistic. Is it true that what Darwin said had very little impact, but that what people thought he said, that is, what they already believed and believed to have been confirmed by Darwin, had an enormous impact?

We appear to be faced with a curious problem, not with the impact of Darwinism but with the absence of impact. The answer, or at least some insight into the possible structure of an answer, lies in the area which I have called covert culture. We need, to begin with, a formula or model which will do something to explain not the rejection of the *Origin*, which is not too difficult to understand, but the perverting and self-deluding acceptance of the work. The following formulation will serve, I think, as a start.

When a human being encounters a stimulus which cannot be fitted into an existent orientation, the fundamental tendency is either to negate the value and importance of the stimulus or to inhibit its effect, either by denying its existence or by misinterpreting it. Beyond this range at one end we have dying of fright and at the other the highly sophisticated effort not to do away with the stimulus but to re-structure the disturbed orientation without misinterpreting the stimulus. The two extremes are fairly rare, and perhaps, if we include the entire human race, equally rare. The arrival at a new high-level orientation requires high intelligence, a personality strong enough to endure considerable disturbance and frustration without collapsing or putting up defenses,

a high-level culture, and – very probably the most important factor – willingness to endure considerable loss of energy. For maintaining a pattern involves loss of energy, but creating and maintaining a novel pattern, particularly one not reinforced by the environment, involves even greater loss. It is easy now to range the responses to the *Origin* in a kind of order: those who totally rejected it; those who completely misunderstood it; those who incorporated it into their existing set of attitudes by misinterpreting it; and finally those who understood it and subjected their personal cultures to a complete restructuring.

The history of high-level culture is the history of complex and richly-structured orientations. Considering the rarity of thorough-going re-orientations, it is by no means surprising, it is indeed to be expected, that the history of the impact of Darwinism should principally have been a history of rejection or misinterpretation.

By talking of "kinds" of orientation I refer to their function in the culture of the time. At any time a stimulus which conflicts with an existent orientation is disturbing, and Darwin is still capable of being very disturbing. It is difficult to absorb the fact, but for the most part our total culture has still been only very superficially affected by Darwinism. In May 1959 I was interviewed over a Philadelphia radio station on the *Origin*; it was a program in which the listeners are encouraged to telephone in questions and comments. The first call was from a woman to whom Darwin was a vicious man, ideas about biological evolution worthless, and I an ignorant and evil person for encouraging the young even to be aware of such notions. I must confess to having been a bit startled that the cultural conditions responsible for the Scopes monkey trial were still so active. The promptness and vehemence of her message were interesting, and equally interesting was her refusal not only not to consider reading the *Origin* but even to say why Darwin and I were so utterly wrong and bad. Similarly a recent book by Gertrude Himmelfarb, written at a higher cultural level, attempts to dismiss Darwin by raising evolutionary problems long since out of date and, most significantly, by appealing to the authority of Kierkegaard and Bergson and by ridiculing Darwin's metaphysical knowledge and abilities. In both instances one has a feeling that the anger and, in the second case, the sophisticated contempt, conceal fear. Certainly, the *Origin* has always displayed an extraordinary capacity for arousing anger and fear.

The reason for the fear is to be found in the fact that between any orientation and a re-orientation comes a stage of dis-orientation. An orientation functions as a means of reinforcing the sense of identity, and it would seem that another way of referring to the sense of identity

is to speak of the sense of value. (I mean the conviction that life is worth the trouble it takes to live it, as irrational a proposition as one could well arrive at.) For instance, the denial of the adequacy of a religion or a metaphysic, since it implies a denial of the system of values apparently derived from it and dependent upon it, involves guilt as well as fear. Anger, fear, guilt, the loss of value and values, and the loss of identity were therefore the great themes of what I have called the literature of Negative Romanticism. In Coleridge and in Byron, for example, the whole nexus of emotions is symbolized by the inability to love, inversely expressed in Wagner's *Flying Dutchman* by the inability to be loved.

This may seem a long way from Darwin, and yet I think it is not. Reading from bottom to top, it is possible to discern a limited range of religious and metaphysical orientations current in 1859. The lowest level of higher culture was occupied by the believers in Providence. Such a "metaphysic" is perhaps the most primitive way of maintaining stability of personality and the sense of value in the face of the unpredicted and the accidental. It simply states that accidents are not accidents; they are willed by a Deity who controls, from behind the scenes, the sparrow's fall. Next comes the notion of transcendent order, of which the observed world is only the dark and confused shadow. A step higher, at least historically, is immanent order, Natural Law or cause-effect Determinism. Both were often enough the underpinning of an evolutionism. Next came what was perhaps the most modern, confined, I think, to a very few individuals (or at least to very few who left documents), of whom Browning was, or was to be after 1859, the most important. I would call it Radical Romanticism. It assumed, as Browning put it as early as *Sordello*, that great and wickedly neglected poem, that any metaphysic is only temporary, that as soon as we arrive at a solution to a metaphysical problem, or any problem, that very solution immediately gives birth to a new problem and is incorporated within it. This is essentially a psychological and skeptical metaphysic, not to be confused with Hegel's, which is both less subtle and more rigid. Its classic statement was given by Rimbaud with his realization that a new orientation could only be arrived at by a willed dis-orientation totally devoid of any expectancy, even, of a new orientation. Finally there was the skepticism descended from Hume, which led a very faint life indeed, and was generally felt at the time to be quite unendurable.

Dismissing the strategy of outright rejection, we can for each of these deduce first an incorporation of the *Origin* by perversion and sec-

ond a thorough-going dis-orientation; a re-orientation can take any form: Darwinian, Darwinistic, or something quite disconnected from either. The *Origin* implied at least four fundamental orientations: the biological world, and by implication the entire empirical world, is a complexity beyond our comprehension; it is possible to create mental constructs which make sense out of the data; these constructs are most satisfactory if characterized not by straight-line cause-effect thinking but by ecological thinking, that is, by the observation of patterns of relationships within and among fields abstracted from empirical reality; one can hope for increasingly complex and reliable constructs.

Providential thinking incorporated the *Origin* by ignoring most of this and seizing upon the accidental nature of the emergence of new species. It triumphantly insisted that the only possible explanation of such emergence was the operation of the Providential Will. But even that much of the *Origin* was disastrous to a Providential thinking which perceived that the emergence of new species with its waste and suffering could not possibly be reconciled with a beneficent Providence. Hence the bleakness and despair of those whose Providentialism was shattered and who were unequipped to discover a new orientation. This seems to have been what happened to Yeats, and before him to Morris. The Prologue to *The Earthly Paradise* presents a very Darwinian world, beyond comprehension, filled with disconnected gratification and suffering and horror, and populated with a wide variety of false gods.

The effect of the *Origin* upon the orientation of Transcendental Order was a little more subtle. Whereas not many Providentialists were able to exploit Darwinism, England and the West in general, in the post-*Origin* years, saw an immense revival of Platonism and its related and derivative orientations. It was essentially a defensive reaction, and the dislike of Darwin exhibited by such Platonists as More and Babbitt is an example of how powerfully ideas of transcendental order were challenged. The *Origin* gave science an enormous new vitality and status, and that in turn gave the various anti-scientific transcendentalisms immense new energy. But Darwin's uncompromising empiricism meant that if the *Origin* were fully accepted, Platonism and idealism, like Providentialism, crumbled. There is perhaps no logical reason why they should have crumbled, but we are here dealing not with logical relations but with covert culture, which works by half-unconscious and intuitively perceived consistencies. The rigid idealist with profound emotional commitments to his idealism simply erected higher and thicker defenses. The flexible and non-dogmatic idealist was disturbed.

As we have seen, the various forms of Natural Law, the forms

of immanent order, including evolutionism, could most easily, of the higher orientations, incorporate Darwinism by misrepresentation and create that odd amalgam, Darwinisticism. It is easy to see today, after the peculiar fate that has overtaken classical physics, that Natural Law is ultimately anti-scientific, although it appears to be scientific on the surface and is, up to a certain point. It is not, therefore, surprising that the effect of Darwinism upon Natural Law was to popularize it. Similarly, there was a strong re-inforcement of cause-effect Determinism. Classical physicists were particularly disagreeable to Darwin, and Huxley himself abandoned his determinism only after some time, perceiving that it was not scientific but Calvinistic. We should remember that the physicists were quite wrong in their insistence, to the great embarrassment of Darwin and to the weakening of his argument, that the earth was not old enough for biological evolution to have taken place. It seems to me unquestionable that the position of Darwin on the nature of science was more advanced than that of the physicists. Yet, because of the superficial content of science in the orientation of Natural Law and Determinism, the dis-orientation effected by Darwin was to increase that content and to convert its metaphysical content into a heuristic function, even before the heuristic concept of Law had been formulated.

It was the "Radical Romantic" who could most readily (with the exception of the Humean skeptic) accept the *Origin*. The Radical Romantic's view of the world as a never-ending series of solutions and dissolutions not only corresponded with Darwin's world, but his concept of moments of vision into the heart of things from which neither moral nor metaphysical nor empirical propositions could be derived was not too far away from Darwin's conception of scientific knowledge as not final. Nevertheless, because of the instability of holding a truth which is expected not to remain a truth or to be eventually revealed as unreliable, Radical Romanticism constantly threatens to turn into a stable metaphysic. But in spite of this weakness, it created the modern environment in which science, the scientific orientation, and skepticism could flourish. Part of the reason for this lay in the fact that it was more interested in patterns than in cause-effect relationships. The Radical Romantic in his moments of vision tended to see truth as a pattern of relationships.

It is thus no accident that Nietzsche, for all his revilings of Darwinisticism, appears in a post-Darwinian world, and that Existentialism, the most recent form of Radical Romanticism, should have followed not long after, built as it is upon Nietzsche and an early Radical Romantic, Kierkegaard. As the case of Browning shows, Radical Romanticism was

the least disturbed of all orientations in the post-*Origin* period (except, again, for Humean skepticism, as revived not only by Huxley but far more profoundly by Ernst Mach).

My purpose in this essay has been to create a construct by which the impact of Darwinism might be investigated rather than actually do the investigation itself, and I must leave it to the reader, if he finds the construct useful, to make what use of it he can. However, I should like to examine a couple of literary documents and a pair of writers in order to show how the construct may be used.

I shall begin with some comments on *The Return of the Native*, written within fifteen years of the first publication of the *Origin*. If in *Under the Greenwood Tree* we seem to be living in a kind of idyllic providential world, and in *Far From the Madding Crowd* in a causal world, in the *Native* we have an entirely different picture. It is true that, to begin with, coincidence, or more accurately, accident, is indeed the means whereby the story is carried on.* The vision that we live in a world in which the unexpected, the unanticipated, the totally unpredictable rules our lives can be unbearable, when it is insisted on. However, Hardy holds the story together from the outside. Behind the series of unpredicted events, providing not a causal but a patterned inevitability, lies the cycle of the year. The lovers meet as the sun is turning north, their love grows as the sun moves northward, it is consummated in marriage very nearly at the summer solstice, by September it has begun to go wrong, and the final tragedy occurs in late November, when once again the sun is sinking towards its southernmost point. This pattern is further reinforced by subtle references to the time of day, particularly in the latter part of the novel, in which the crucial September scenes

* Some incidents from my own experience in teaching this novel are particularly pertinent. Some twenty years ago in a Southern college which shall be nameless I had the task of trying to persuade a group of badly educated freshmen to grasp the book. They objected to it violently. They had been taught in high school that a good novel or play does not depend for its plot progressions and resolutions on coincidence. Hardy's novel does depend on coincidence. Therefore it is a bad novel. Again and again I explained that that was Hardy's whole point. They always said that they understood, but at the next meeting of the class the same objection would be brought up again. I fear I failed. Ten years later I was relating my experience to a colleague in a Northern university. He was most condescending, not only to freshman and Southern freshman, but especially to me, for my failure in not getting the students to grasp the point. "However," he added, "there's one thing in Hardy *I* can't swallow. In *Tess* everything hinges on the fact that the letter goes *under* the carpet." Here are two good examples of how profoundly the orientation of causality has penetrated our culture. Reading detective stories is a kind of a metaphysical ritual for reinforcing it. But the *Native* clearly does not belong to that kind of orientation.

take place in the afternoon and the final November scenes not long after sunset. But there is a further element of organization: the opening description of Egdon Heath, the careful topography, without which the catastrophe cannot be understood and could not have happened in the way it did, the mixed racial background of Eustacia, Clem's alienation from his native attitudes by his residence in Paris – a thousand big and little elements which show the relation of the characters to their cultural and natural environment and equally their violations of those relations. One great theme of the novel is the ecological relation of personality to, in the anthropological sense, the landscape.

All three of these elements – accident beyond our anticipation or comprehension, control of structure by pattern, ecological thinking – are Darwinian. To be sure, Hardy might have derived the notion of the seasonal cycle from the folklorists, anthropologists, and archaeologists of his day, but they in their turn had been strongly influenced by the *Origin*. And no doubt there is already something of Schopenhauer and von Hartmann in the work. But these last influences on Hardy seem to me to govern more the later novels. The *Native* exhibits, to my mind, a profoundly Darwinian orientation, even to the implication that we can never fully understand the world in which we live, and our relation to it.

My second example comes from something apparently highly remote, though almost contemporaneous, Pater's *Renaissance*, a superbly structured book, from the opening Preface to the famous Conclusion. But its structure – and this is perhaps why it has been taken to be merely a collection of only historically related essays with a conclusion tacked on – is not the structure of logic, which, it is to be remembered, was thought of as descriptive of the cause-effect structure of reality. Rather it is organized, first by the recurrence of themes, and second by Pater's delicate revelation that in each of the individuals or works discussed is to be found a similarity of pattern, the orientive patterns of the modern world, gradually emerging from an early gothic love story through Leonardo to Winckelman, and crystal clear only in the Conclusion. Here again, I think, is a Darwinian kind of thinking, but much more Darwinian is another ingredient in the basic structure of the Conclusion.

I have pointed out that one of the shocks administered by Darwin to theology was that the one thing which remained unattacked in the eighteenth- nineteenth-century argument from design, or natural theology, was the perfect adaptation of organism to environment, a subject on which the Bridgewater lectures had been so wearisomely explicit. But Darwin, of course, had revealed that the whole mechanism of nat-

ural selection and the origin of species depended upon the imperfect, or less than perfect, adaptation of organism to environment. Now this, I believe, is the basic assumption for the argument that we should burn with a hard, gem-like flame. In terms of the history of Romanticism, Pater was applying to art, to religion, to metaphysics, to morality, exactly the pattern of the momentary spots of time which Wordsworth had applied to nature. The problem of the disoriented or negative Romantic was to re-create the sense of value by establishing an orientation which would relate the self to the universe. Hence Wordsworth makes a romantic and psychological adaptation of the argument from design, which was already current, when in the passage from the *Recluse* quoted in the Preface to the *Excursion* he speaks of how exquisitely the mind is adapted to the universe. Because of this adaptation we can make the transition from self to nature and perceive it as a symbol of the Divine, or value. But Pater, the post-Darwinian who had lost his Christian faith even before the publication of the *Origin*, can make no such leap. As he sees it, the mind, the emotions, the personality are not adapted to the conditions of human existence. Rather, ideas, whether metaphysical, theological, or moral, are instruments of adaptation by which we adjust ourselves to the world. They are not final truths, nor ever can be. They are, rather, adaptational illusions, but far more satisfactory than any of these is the work of art, since it gives the highest value to the disconnected moments of the personality as they appear and does not pretend to be true. Art, therefore, is the most successful of our adaptational mechanisms. Admittedly, this is in part to translate Pater into Darwinian terms, but its seems irresistible that Pater is saying that the world cannot be known and that our modes of knowing and experiencing are instruments of the observer, not modes of comprehending metaphysical or even scientific truth.

The case of Pater indicates how delicate and subtle is the problem of Darwinian influence. There is always the possibility of cultural convergence. Perhaps he could have come to these conclusions had it not been for Darwinism, but it seems most unlikely. At any rate, that is a question that cannot be answered. We can only say that in the *Origin* is a locus of a novel orientation, and that it seems not to be a misinterpretation of Pater to discover the same orientations in his work. Presumably, the Darwinian orientation entered into Pater's covert culture and left important traces in the Conclusion to the *Renaissance*.

In conclusion, we can examine briefly two figures both currently of great cultural importance, Nietzsche and Freud. In Nietzsche's con-

cept of the will to power (by which he did not mean political tyranny) we have a highly Darwinian notion of the desire to control our environment. This desire has no transcendental source. Rather its roots are physiological within the individual and ultimately biological. The will to power is a way of stating the endless and never-to-be-consummated drive to adapt ourselves to our environment. Hence, for all the suffering in the world, the power of joy is deeper than suffering. The drive towards a sense of value is greater than all its frustrations. The "will to power" thus brings Darwin's ecological vision within the reach of human psychology and human morality.

In the same way Freud, who refused to read Nietzsche because he feared his own originality might be thereby damaged – thus showing that he knew something about him – presents neurosis not only as the repetition of a pattern but something from which everyone suffers and which can never be entirely overcome. Hence analysis is endless. We can never make the perfect adaptation. The only complete adaptation is to adapt our neuroses to those of others in our social environment, to conform; but that is a counsel of despair. It is in the endless effort to solve the unsolvable that Freud finds his peculiar grim optimism closely resembling both Darwin's and Nietzsche's. Yet in Freud we find another example of Darwinisticism, of turning a Darwinian perception into a metaphysic. Implicit and explicit in Freud is an idea which has been so over-developed by naïve dogmatic Freudians that it completely dominates their thinking. That notion is that in the psychological world there are no accidents, that all is determined, that cause and effect rule everywhere, and that everything in the individual's psychological past can be understood and accounted for.

It is not surprising that a Freudian metaphysic has become most popular in those areas and levels of Western culture which, to a considerable degree through the impact of Darwinism and Darwinisticism, have been most thoroughly secularized. It has filled the vacuum left by the departure of religion and metaphysics; but the drives of Providentialism are still with us. This new metaphysic, now so popular at middle-class American cultural levels, is only another example of how Darwinisticism has perverted Darwinism, and how the *Origin* has had far less impact than have other orientations, both already existent and more recently emergent, which have mis-used and mis-interpreted that great book for the purposes of their own survival.

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DARWIN'S IMPACT ON BRITISH METAPHYSICS

THE INFLUENCE OF A particular man upon a wide area of thought – how is that to be estimated? For he will himself have been profoundly influenced by his predecessors and by the intellectual atmosphere of his time, and these influences play upon his successors just as they did upon him. Thus, for example, although Charles Darwin was always inclined to deny that he had learnt anything from his grandfather Erasmus Darwin or from Lamarck, the fact remains that they must at least have familiarised him with the idea of evolution.[1] Then, once Darwin had made the idea of evolution respectable, Lamarck's reputation rose too, and exerted an influence theoretically independent of (although historically made possible by) the impact of a strictly Darwinian biology.

Furthermore, men sometimes appealed to the authority of Darwin as they might now appeal to the authority of Einstein, merely because some concept of evolution or development – as now some concept of relativity – plays a part in their speculations, even if their "evolution" is quite remote from Darwin's biology in character and spirit. It is hard to be confident that this or that innovation within biology itself derives

[1] For a sensible discussion of this issue, see the second appendix to Nora Barlow's edition of *The Autobiography of Charles Darwin* (London, 1958).

from Darwin; but the difficulty in estimating influences is particularly acute when the influence is inter- rather than intra-disciplinary.

One can fairly confidently ascribe, say, a particular set of experiments upon orchids by one of Darwin's successors to Darwin's influence, for the whole work will be intimately related, in its details, to Darwin's. Philosophers, on the contrary, were interested only in the most general implications of Darwin's work; he helped, in certain respects, to open their minds to new possibilities, or gave them greater confidence in developing possibilities which had already occurred to them. An influence of this sort can only be described impressionistically; the present essay does not lay claim to the sort of exactitude which can reasonably be demanded, say, in a study of Malebranche's influence upon Hume – for that is not the sort of influence which Darwin exerted. We are here concerned rather with the way in which Darwin encouraged and reinforced certain philosophical tendencies in Great Britain than with direct influences of a more technical sort – with his impact upon such diverse philosophical tendencies as agnosticism, naturalism, and idealism, which he certainly did not create but nevertheless helped to shape.[2]

Not that Darwin was uninterested in philosophical questions. As a young man he delighted in the study of William Paley's theological writings, which Cambridge then required of all its students;[3] in later life he occasionally embarked on a course of metaphysical reading; now and then, more particularly in his correspondence, he will comment on an issue in the philosophy of science.[4] But he was no philosopher. "My power to follow a long and purely abstract train of thought," he confesses, "is very limited. I should, moreover, never have succeeded with metaphysics and mathematics" (*Autobiography*, p. 140). Not that he was by any means opposed to speculation; he was extremely conscious – more conscious, perhaps, as he grew older – of the role of general ideas in science.[5] But long passages of purely theoretical deduction, so characteristic of mathematics and philosophy, did not lie within the range of his abilities.

His mind was essentially concrete. Productive in the formation

[2] In this article, I have presumed a general acquaintance with the nature of the philosophical views to which I refer. For a fuller exposition see my *A Hundred Years of Philosophy* (London and New York, 1957).

[3] *Autobiography*, p. 59. Thus he was well acquainted with the classical presentation of that "argument from design" his own discoveries were to undermine.

[4] At Cambridge, Darwin had long conversations with William Whewell, and later read and admired his writings on the philosophy of science (*Autobiography*, pp. 66 and 104). His own ideas in this field are very close to those expressed by Whewell.

[5] See in his *Autobiography* his resentment at the criticism that "he is a good observer, but has no power of reasoning" (p. 140), and Lady Barlow's comments in the second appendix (pp. 157-159).

of hypotheses, he preferred them to be of a type which lent themselves to immediate testing by observation: hypotheses testable, let us say, by direct examination of the processes of fertilization in orchids. Even his theory of natural selection he took to be of that directly-testable kind: "All my notions about *how* species change," he once wrote, "are derived from long-continued study of the works of (and converse with) agriculturists and horticulturists."[6] He thought of himself, simply, as extending to a wider field principles which were already quite familiar to the breeders of plants and animals.

Philosophical propositions are not, in this sense, concrete. Consider, say, the positivist principle that "all knowledge is founded on experiment and observation." No doubt this principle is closely related to science: it gains moral support, we might say, from the success of science. Yet, at the same time, it cannot itself be tested by experiment and observation: no experiment could overthrow it, and no series of experiments, however long continued, can do anything to establish its truth. Even though its wide acceptance in the nineteenth and twentieth century is obviously connected with the continued extension of science into new fields of inquiry, the positivist principle is not related to the growth of science as a direct deduction from it. Rather, one might say, the growth of science creates an atmosphere in which more and more people are prepared to recognize its initial plausibility, and to listen to arguments (of a philosophical, not a directly scientific, sort) in its favour.

This, in general terms, is Darwin's importance for philosophy. One can understand why Wittgenstein wrote: "The Darwinian theory has no more to do with philosophy than any other hypothesis of natural science."[7] The theory of natural selection is not, in itself, a philosophical theory: the *Origin of Species* is not a contribution to philosophy as Wittgenstein's *Tractatus* is. Yet if one looks at the matter in a broader, historical way, Wittgenstein was wrong. For Darwin's theory profoundly disturbed the traditional picture of man's relation to the world in which he lives and moves and has his being. It made it not impossible, certainly, but much less plausible to insist on man's ontological uniqueness; not impossible, again, but less simple, to believe that the world was designed by a supernatural being. And again, while Darwin did not invent those ideas of development and continuity which play so notable a part in nineteenth-century philosophy, his scientific theories bore wit-

[6] Letter to Asa Gray in *Life and Letters*, ed. Francis Darwin, 3 vols. (1887), II, 79.
[7] *Tractatus Logico-Philosophicus* (London, 1922), §4.122.

ness to their profound importance for the understanding of the world.

Nineteenth-century philosophers were well aware of the explosive effect of Darwin's teachings. Darwin presented them, not with a philosophical theory, to be sure, but all the same with a challenge.[8] The effect of his work on logic, even, should not be underrated. For there was one form of reasoning which had traditionally been supposed to be of quite peculiar logical importance: the argument from design. The argument from design began from what was taken to be a matter of simple observation – the presence of design in natural processes – and concluded from this characteristic of natural processes to the existence of a supernatural being, a God conceived as a Great Designer. Thus the argument from design stood as the supreme, perhaps the only, example of a type of reasoning which made a transition from the observation of natural processes to conclusions about the existence of a supernatural order.

Admittedly, Hume had already argued that such reasoning is bound to be fallacious, that from the observation of natural processes it is impossible in principle to conclude to the existence of a non-natural being. But his arguments, in England at least, were ignored: Paley was zealously studied while Hume's works gathered dust – unread even by empiricists. For all that he does not directly discuss the philosophical issues involved, Darwin, not Hume, shook men's faith in the argument from design; if they came to read Hume, as Huxley did, and to appreciate the force of his arguments, this is because Darwin had helped to prepare their minds to receive them.[9]

His importance in this respect comes out very clearly in Mill's essay on "The argument from marks of design in Nature."[10] Alone

[8] A comparison with William Harvey's impact on seventeenth-century science will bring out the general character of nineteenth-century reactions to Darwin. See my "William Harvey and the Philosophy of Science," *Australian Journal of Philosophy*, XXXVI (1958), 85-94.

[9] Of course I am talking here only about England. In Germany the story was very different because Kant had at once recognised the force of Hume's *Dialogues* and had incorporated his main arguments into his *Critique of Pure Reason*. But it took a long time for Hume's arguments to have any real effect in England. See N. K. Smith's edition of Hume's *Dialogues Concerning Natural Religion* (Edinburgh, 1935), especially pp. 30 and 75 of his Preface. Although writers like Sir William Hamilton professed to be Kantians, serious study of Kant in England dates back only to Edward Caird's *The Philosophy of Kant* (1877). Interestingly enough, it was the idealist Thomas Henry Green, himself profoundly influenced by Kant, who made the first substantial British study of Hume in his introduction to *The Philosophical Works of David Hume*, ed. T. H. Green and T. H. Grose (1874).

[10] First published (posthumously) as a chapter in his "Theism," one of the *Three Essays on Religion*, ed. Helen Taylor (1874), but written in the years 1868-70. Mill refers in these essays to Hume "On Miracles," but not at all to the *Dialogues Concerning Natural Religion*.

amongst the traditional arguments for the existence of God, the argument from design, Mill says, has "a really scientific character." It exemplifies what in his *Logic* Mill had called "the method of agreement"; the weakest, he admits, of the inductive methods, but of by no means negligible force. On the validity of the argument from design, he concludes, the scientific case for theism entirely rests.

Yet this argument would collapse, Mill confesses, if what he calls "recent speculations" (p. 172) – Darwin's theory of natural selection – could be made good. If Darwin is right, no one can any longer pretend to prove theism on scientific grounds. A man might still be a theist as a matter of personal faith but "natural theology" would be wholly worthless.

Of course, natural theology already lived a somewhat precarious existence, attacked by the more dogmatic theologians as placing too much stress on reason, by sceptics because it professes to prove what, in their opinion, never can be proved: the existence of supernatural beings. Mill's younger contemporary, the theologian Henry Mansel, devoted his much-discussed Bampton lectures, *The Limits of Religious Thought* (1858), to a demonstration of the impossibility of our having any knowledge of God as he is in himself. Mill was trying to tread a middle path between Mansel's agnosticism of the right[11] – derived from Kant *via* Sir William Hamilton – and Huxley's more Humean agnosticism of the left; as men came to be convinced that Darwin was correct, Mill's own admissions helped to persuade them that this path was closed for ever. The decline of natural theology in the late nineteenth, and our own, century derives from a number of different sources, but Darwin's work certainly played a part in it.[12]

Darwin himself was to write: "The mystery of the beginning of all things is insoluble by us; and I for one must be content to remain an Agnostic" (*Autobiography*, p. 94). He had abandoned orthodox Chris-

[11] Which Mill particularly abominated. See his *Examination of Sir William Hamilton's Philosophy* (1865). I mean by "agnosticism of the right" the view that criticises the possibility of arriving at knowledge of God by rational demonstration, only to conclude that the true source of such knowledge must be Revelation. This was to become the leading tendency in theological thought as a result of the collapse of natural theology. An agnosticism of the left, on the other hand, is not prepared to see in Revelation a source of knowledge alternative to Reason. The agnostics of the right are ardently Christian; the agnostics of the left, on the contrary, are scarcely distinguishable from atheists.

[12] Thus according to Noel Annan, Darwin's *Origin of Species* finally lost Leslie Stephen his faith. See Annan's *Leslie Stephen* (London, 1951), ch. v, on the whole issue, and Leslie Stephen's own *An Agnostic's Apology* (1893). Cf. T. H. Green's view, in his *Lectures on Logic*, Philosophical Works (1885-88), II, 78, that Darwin's arguments completely destroy "the old natural theology."

tianity many years earlier,[13] but might well have been content to rest in the vague theism of his middle years, had it not been for Huxley's influence. Certainly Huxley by no means originated agnosticism. But at the same time he was right in thinking that there was a natural alliance, even if Darwin did not at first perceive it, between Darwin and agnosticism. For by destroying the argument from design Darwin did not disprove God's existence, but cut away the only argument in its favour which had any appeal to those who accepted the positivist doctrine that all knowledge derives from the observation of natural processes. Thus, it seemed to follow, one should rest content with saying that God's existence can neither be ruled out or proved – the characteristic agnostic doctrine.

The alliance between evolution and agnosticism is still further exemplified in the work of Herbert Spencer, but in a way which particularly emphasises how easy it is, and how wrong it would be, to exaggerate the philosophical influence of Darwin's work – or, at least, its influence in a direct and obvious sense. For Spencer's philosophy is in most respects non-Darwinian. His agnosticism is of the right rather than the left; from Hamilton and Mansel he derived his conception of religion as a "consciousness of an unknowable Power." Indeed, large sections of the text in Part I of Spencer's *First Principles* – entitled "The Unknowable" – consist of direct quotations from these two writers; and in the prospectus he issued to potential readers of his *System of Philosophy* he is content to describe himself, with a not-altogether-characteristic modesty, as "carrying a step further the doctrine put into shape by Hamilton and Mansel."

As for evolution, Spencer was an evolutionist before Darwin. In the Preface to the fourth edition of his *First Principles* he protests against the widely-held assumption that his metaphysical ideas are simply a generalisation of Darwinian biology. He points out that his essay on "Progress: its Law and Cause," which contains the gist of his "philosophy of evolution," appeared in the *Westminster Review* of April 1857, two years before the *Origin of Species*. Huxley tells us, indeed, that Spencer had tried to convert him to evolution as early as 1852.[14] But Spencer could give no account of the manner in which species are transmuted,

[13] In an undated letter he wrote thus to Francis Galton: "I gave up common religious belief almost independently, from my own reflections" (unpublished paper, Cambridge University Library, as quoted in William Irvine, *Apes, Angels, and Victorians* [New York, 1955], p. 109 n.).

[14] See Huxley's essay on "The Reception of the 'Origin of Species'," published as ch. v of Darwin's *Life*, II, 188. Spencer's "evolution" had been and remained Lamarckian in its tendency.

and therefore Huxley felt free to ignore what, from his point of view, was no more than a metaphysical speculation. Spencer himself admits in his preface that without Darwin's doctrine of natural selection, there would be "a serious gap in the general argument" – although a footnote on "natural selection" in the main body of the text (6th ed., p. 362 n.) is considerably more grudging in its admission of Darwin's originality and importance. The fact remains that this "gap" would not have deterred Spencer from his metaphysical constructions.

But if Darwin's views had very little effect upon the general structure of Spencer's philosophy, they had a notable effect upon its public reception. Spencer was naturally indignant that he should be regarded as a mere popularizer of Darwinism; the fact remains that his enormous popularity derived, at least in part, from the widespread belief that his metaphysical ideas had a "sound scientific basis" – the Darwinian theory of natural selection.[15]

Not everybody was prepared to agree, however, that Darwin's evolutionary theory naturally led to agnosticism. On the one side, there were those who welcomed it as lending support, in their opinion, to the Calvinistic conception of the universe, in opposition to any sort of humanitarianism. James McCosh, a Scottish philosopher who migrated to Princeton, was one of the most notable members of this school. " 'The whole creation groaneth and travaileth in pain together until now.' Our academic theists were refusing to look at our world under this aspect," he wrote in his *Christianity and Positivism* (1871), "Even some of our sentimental Christians were turning away from it. It is a curious circumstance that it is science that has recalled our attention to it" (p. 62). For those who were prepared to rest their belief in God's existence on the authority of the Bible and not on some sort of quasi-inductive argument from the "design" exhibited by natural processes, Darwin's work was less devastating than it was for men like Stephen and Mill. Such theologians could readily enough, especially if they were Calvinists, adjust their conception of Providence so as to incorporate Darwin's teachings. But although there were Christians in England who welcomed Darwin's theories on somewhat similar grounds, Calvinistic Darwinism was on

[15] Darwin himself saw clearly that this was not so. Certainly, in a letter to Ray Lankester on 15 Mar. 1870 (*Life*, III, 120), he goes so far as to say that Spencer "will be looked at as by far the greatest philosopher in England; perhaps equal to any that have lived." But this may be a somewhat ambiguous compliment. In a letter to John Fiske (8 Dec. 1874, *Life*, III, 193) Darwin writes that "with the exception of special points I did not even understand H. Spencer's general doctrine"; and again that "such parts of H. Spencer as I have read with care, impress my mind with the idea of his inexhaustible wealth of suggestion, but never convince me." Perhaps this is the most Darwin expected of a philosopher!

the whole a peculiarly American phenomenon, just as the traditional Scottish philosophy in general continued to flourish in America when, under the impact of Mill's criticism on the one side and the criticism of the Idealists on the other, it had very largely died out in England.[16]

Of more importance in England was the emergence of what Disraeli called "the religion of science." Monsignore Berwick in *Lothair* (1870) detected two distinct ingredients in this "religion"; "Instead of Adam, our ancestry is traced to the most grotesque of creatures: thought is phosphorus, the soul complex nerves, and the moral sense a secretion of sugar."[17] The religion of science incorporated, that is, "medical materialism," deriving from such German writers as Ludwig Büchner and Karl Vogt – author of that famous phrase, "the brain secretes thought, just as the liver secretes bile" – and the evolutionary theories of the Darwinians.

Public opinion commonly ascribed a religion of this sort to Huxley, Tyndall, and Clifford – a "religion" at once atheistic, materialistic, and deterministic. But for all that he had naturalised in England Büchner's epiphenominalism – even down to the notorious steam-whistle analogy – Huxley denied that he was a materialist; indeed, if he had to choose between materialism and idealism, he would, he tells us, call himself an Idealist. Nor was he an atheist; but rather an agnostic. As to determinism, he said, that was a perfectly respectable view, with a long theological tradition behind it. Lenin laughed at this disclaimer: "in Huxley," he wrote, "agnosticism serves as a figleaf for materialism";[18] and many an ecclesiastic was of Lenin's opinion. But Huxley, the most infuriating of controversialists, was obdurate on the point.

One has to turn to Germany, to Ernst Haeckel's *Riddle of the Universe* (1899) – almost immediately translated for, and widely read

[16] Huxley did at times suggest that his determinism was a perfectly respectable, because Calvinistic, doctrine. See also David Lack, *Evolutionary Theory and Christian Belief* (London, 1957) and ch. iii in Rudolf Metz, *A Hundred Years of British Philosophy* (London and New York, 1938). On Calvinistic Darwinism, and on the persistence of the Scottish tradition in America, see H. W. Schneider, *A History of American Philosophy* (New York, 1947), especially ch. iv. §30-32.

[17] *Bradenham Edition of . . . Disraeli* (London, 1926-27), XI, 145. Notice that as early as *Tancred* (1847) Disraeli depicts evolutionary ideas, deriving from geology, as being a subject of polite conversation (X, 112-113). The precise subject of discussion is a work entitled, significantly enough, *Revelations of Chaos;* the context makes it clear that this is Disraeli's renaming of Robert Chambers' *Vestiges of the Natural History of Creation*, a work which went into fifteen editions between 1844 and 1880. Chambers maintained that "the form of progress of organic life upon the globe . . . is that the simplest and most primitive type gave birth to the type next above it, that this again produced the next highest, and so on to the very highest, the stage of advance being in all cases very small" (2nd ed., 1844, p. 322).

[18] *Materialism and Empirio-Criticism* (1908), authorised English ed. of Lenin's works (New York, 1927-32), XIII, 172.

by, the English reader – for an explicit formulation of what its critics like to refer to as "nineteenth-century materialism" – as if materialism were a peculiarly nineteenth-century phenomenon. Haeckel was himself a distinguished biologist, and Darwin is his hero. But he had none of Darwin's hesitancies. He would certainly not have described himself as an agnostic; indeed, he saw in agnosticism an attempt to blur over the real issues. For him natural selection is a "mathematical necessity of nature that needs no further proof," and "evolution is the magic word by which we shall solve all the problems that surround us." No English philosopher went so far in ascribing philosophical significance to Darwin's teachings.

The philosophers with whom we have so far been concerned were all – with reservations in Spencer's case – enthusiastic Darwinians; men who were delighted by, rather than repelled by, what they took to be the naturalistic and anti-theological implications of his work. But, of course, this was by no means the invariable reaction, and Darwin's influence is as manifest in those who fought against his teachings as in those who fully accepted them. They, too, had to come to terms with Darwin, and in ways which manifest his importance for philosophy.

The most obvious response of this kind came from those who, while they accepted the idea of evolution, rejected the view that it operated through natural selection, seeing in it, rather, the operations of a "life force." This point of view was vigorously expressed by the publicist and novelist Samuel Butler in such works as *Life and Habit* (1877), where he reverted to the Lamarckian conception of evolution. At first an admirer of Darwin's, he turned violently against him, partly for personal reasons,[19] partly because of a profound distaste for what he took to be Darwin's "purely mechanical" account of evolution. Butler's ideas were adapted by Bernard Shaw to his own purposes in his *Man and Superman* (1903) and *Back to Methuselah* (1921). But his influence in the narrower sphere of professional philosophy was not considerable.

A similar view, however, had a somewhat better fate when it was maintained by the French philosopher Henri Bergson, of whose many works *Creative Evolution* (1907) is the one most relevant to our present purpose. Darwinism, perhaps for patriotic reasons, had never won any great favour in France;[20] it was regarded, on the whole, as just the sort

[19] See the lengthy appendix on this subject in Darwin's *Autobiography*.

[20] See Huxley in Darwin's *Life* (pp. 185-186). In Emile Bréhier's *Histoire de la philosophie* (Paris, 1950), for instance, Darwin's name appears only in connexion with British and German philosophical speculation.

of view an Englishman *would* maintain, crassly mechanical and materialistic. One of the more sympathetic of Darwin's French critics, the biologist Marie Jean Pierre Flourens, wrote of the *Origin of Species* thus: "At last Mr. Darwin's work has appeared. One cannot help being struck by the talent of the author. But what unclear ideas, what false ideas! What metaphysical jargon clumsily hurled into natural history! What pretentious and empty language! What childish and out-of-date personifications! Oh lucidity! Oh French stability of mind, where art thou?"[21] So there was a welcome for Bergson's attack on Darwinian conceptions; fashionable audiences, of a sort more often to be found in the Opera House than the lecture room, flocked to hear his attacks not only on natural selection but on the very idea that the ordinary methods of science can be applied to the understanding of living processes.

In England, too, there was in the early years of the century a vogue for Bergson; his works were freely translated, commented upon, and read.[22] Popularising philosophers like C. E. M. Joad absorbed his ideas into their eclectic systems. To attack the very possibility of science, while at the same time accepting the (by now) generally accepted idea of evolutionary progress, was to make the best of both worlds.

At the same time, Bergson's philosophy was to have little substantial effect on the general character of British metaphysical thinking. The view of the philosopher-biologist C. Lloyd Morgan that Bergson's "philosophy" could more properly be described as a form of poetry expresses the common reaction to Bergson of British philosophers, trained as they are to pay proper respect to science. Even the Idealists, as we shall see, kept poetry out of science, although they did not believe that science can lead us to "ultimate reality."

C. Lloyd Morgan's own theory of "emergent evolution" was much more to the taste of those who followed in the empirical tradition. The conception of "emergent evolution" goes back at least as far as G. H. Lewes' *Problems of Life and Mind* (1874-79). Darwin, in the intellectual context in which he found himself, naturally insisted on the continuities between the human and the non-human; "emergent evolution" accepted this continuity but – as against Haeckel and his kind – also insisted that evolution involved the emergence of quite novel properties: properties founded on what preceded them, but in no sense reducible to their

[21] Quoted in *Life*, III, 31, from M. J. P. Flourens, *Examen du livre de M. Darwin sur l'origine des especes* (1864).
[22] Thus works on Bergson were published by H. W. Carr, J. A. Gunn, A. D. Lindsay, J. McK. Stewart, between 1911 and 1920.

predecessors. As worked out by Samuel Alexander in his *Space, Time and Deity* – not published until 1920 but foreshadowed in a series of articles over the preceding decade – "emergent evolution" was thoroughly naturalistic, quite ready to absorb Darwin's views, but at the same time a great deal more sophisticated intellectually than earlier evolutionary philosophies. In one form or another, it won the support of many British philosophers, including such staunch adherents of the "analytic" school as C. D. Broad. At the moment, interest has shifted from this sort of philosophical issue, but in so far as British philosophers are still thinking about the matter, some form of "non-reductive" evolution is what they would largely take for granted.

In general, however, the great days of biologically-toned philosophies are over; the naturalism of philosophers like Dewey and Santayana, or the quasi-biological metaphysics of Whitehead, would be now criticised just on account of their biological emphasis. "Naturalism," nowadays, is logical rather than biological. The fact remains that Darwin's work undoubtedly stimulated the working-out of naturalistic philosophies,[23] even if, in the opinion of most contemporary philosophers, these laid for a time too great a stress on the human organism, as distinct from the general structure of things. From the point of view of a naturalism which is primarily concerned to insist that "there is no knowledge except of the type commonly called scientific" and that "there are no processes except natural processes," the details of Darwin's work are relatively unimportant.

What matters is that Darwin forms one of a chain of thinkers who helped to extend scientific procedures into fields where they had seemed to be quite inapplicable and to bridge that gulf between man and animal which had once been assumed to be unbridgable. Copernicus, Galileo, and Newton destroyed the supposition that the terrestrial and the celestial belong to different orders of reality; Harvey and Descartes showed that mechanical processes are to be found within the human body; Darwin, that species are no exception to the general processes of change which are characteristic of physical objects, and that men have not the kind of uniqueness they like to ascribe to themselves.

Of those philosophers who, on the face of it, wholly turned their

[23] Particularly, perhaps, in the United States. The transition from biological to logical naturalism is more fully discussed in my *A Hundred Years of Philosophy* (p. 294). British philosophers have very often been naturalistic in outlook, but have rarely wished to describe themselves as such.

back on the naturalistic tendencies of the nineteenth century, the most uncompromising were the group of thinkers commonly known as "the British Idealists," or sometimes, if not altogether accurately, as "the Anglo-Hegelians." In a direct sense, one can ascribe little in their work to the influence of Darwin; yet in a way, their very existence and popularity owes a great deal to Darwin, or at least to the rise of that naturalism in which he was so important a figure.

On the continent of Europe, the rapid development of physical science in the nineteenth century had the effect of breaking the hold, at least temporarily, of traditional modes of philosophising: the German scientist-philosophers made a vigorous attack on Hegel, in particular. Indeed, they turned to the British empirical tradition for philosophical inspiration. Precisely the reverse situation occurred in Great Britain. Alarmed by the rise of naturalism, a good many philosophers hoped to discover in the Continental philosophical tradition a shield against science.

Six years after the appearance of the *Origin of Species*, J. H. Stirling published his *The Secret of Hegel* (1865) in which the theological intention was quite open and unashamed. "Kant and Hegel," he wrote with more enthusiasm than accuracy, "have no object but to restore Faith – Faith in God, Faith in the Immortality of the Soul and the Freedom of the Will – nay, Faith in Christianity as the revealed religion" (Preliminary Notice, p. 22). This is the ground on which he recommended Kant and Hegel to his readers, even if the hopes Stirling aroused were in the outcome to be disappointed.

The Scottish Hegelian, Edward Caird, saw Hegel in a similar light, as being driven by "the desire to restore the moral and religious basis of human existence, which a revolutionary scepticism had destroyed" (*Hegel* [1883], p. 202). Yet, at the same time, the ideas which Caird took from Hegel were distinctly unorthodox, and in a direction which produced a striking convergence between Hegelian Idealism, as he understood it, and the new naturalism.

This convergence is visible in two respects. First Hegel had laid enormous stress upon change and development. Although Hegel's conception of development is very different from Darwin's, Caird found it possible, looking at things from a Hegelian point of view, to speak with considerable sympathy of Darwin because, he rightly thought, Darwin had so greatly contributed to the general acceptance of the idea of development.[24] The popularity of such of Caird's works as *The Evolution*

[24] See also D. G. Ritchie, *Darwin and Hegel* (1893).

of Religion (1893) derived in part from the degree to which Darwin had made evolution a "magic word."

Secondly, Caird is at one with the materialists in denying that there are two worlds: a natural and a supernatural, a material and a spiritual, God and Nature. No doubt, whereas the naturalists asserted that the one world is natural, material, philosophers like Caird hoped to show that it was a spiritual world – that the laws of science are an expression of the human mind, and Nature an imperfect embodiment of Mind. But at least they agreed on its unity.

It has been said that pantheism is a polite form of atheism: to assert that everything is God is certainly to deny that there is a God, as that word is ordinarily understood. And similarly one cannot but be struck by the resemblances between naturalism and the Absolute Idealism of philosophers like Caird and Bosanquet: so concerned are they to insist that there is nowhere a gap between the spiritual and the material, between the human and the natural, that one is often inclined to say – Absolute Idealism is the polite form of naturalism. A contemporary critic complained of one of the Idealists, Bosanquet, that "almost every word Dr. Bosanquet has written about the relation between mind and matter . . . might have been written by a complete materialist."[25]

To those who sought to overthrow Darwin by drawing absolute distinctions between the living and the non-living, facts and purposes, mechanism and teleology, Bosanquet's opposition was uncompromising. Consider, for example, the following extract from a letter to James Ward: "The tremendously sharp contrast which you draw between the inorganic and the organic tendencies, evolution without guidance, and with guidance, and the whole preference of Lamarckism to ultra-Darwinism, strikes at the root of the continuity I love."[26] Here it is nakedly apparent that there is more in common between Darwinism and Absolute Idealism than there is between Idealism and the orthodox Christianity which, it was at first expected, the Idealists would save from the onslaughts of Darwin. Darwin did not create that idea of continuity in which the naturalists and the idealists both rejoiced – but he did much to illustrate its power and to elevate it in popular esteem.

True enough, in Bradley's *Appearance and Reality* (1893), development and change are dismissed as "appearances." "For nothing perfect, nothing genuinely real," he writes in a famous passage, "can

[25] J. E. McTaggart, reviewing Bosanquet's "The Principle of Individuality and Value" (*Mind*, XXI [1912], 422).
[26] *Bernard Bosanquet and his Friends*, ed. J. H. Muirhead (London, 1935), p. 108.

move. The Absolute has no seasons, but all at once bears its leaves, fruit and blossoms" (2nd ed. [1906], p. 500). According to such a doctrine, Darwinism loses all claims to describe Reality as it ultimately is. But this is a fate it shares with every other form of science, and indeed with religion. When it comes to choosing between different forms of "appearance," Bradley is by no means unsympathetic to Darwinism.[27] If his *Appearance and Reality* is in some respects intended to be the final reply to Darwin, it is a reply which concedes that Darwin was right in laying such stress on continuity and unity. Bradley was certainly not one of those philosophers who thought it enough to reaffirm the discontinuities – between natural and supernatural, mechanical and teleological, organic and human – which Darwin had helped to destroy.

Thus if one more obviously sees the influence of Darwin in Bradley's inveterate enemies, the pragmatists, and in particular in the leading English representative of pragmatism, F. C. S. Schiller, than in Bradley it is a mistake to conclude that Bradley's attitude to Darwin was one of mere hostility. Schiller is an enthusiastic evolutionist[28] and his emphasis on the active, striving, individual obviously derives from the biologists. Yet the fact remains that the personal idealism of Schiller and his like is in many respects more remote from the spirit of Darwin than the Absolute Idealism of Bradley – just because the personal idealists are attempting to reinstate the doctrine of the uniqueness of the human being, a uniqueness which disappears as completely in Absolute Idealism as it does in naturalism. "The scientific study of Nature," writes the Hegelian Ritchie, "shows us that not only is nature 'careless of the single life,' but that even the type or species is transitory, that the infinite diversity of kinds and individuals does not exclude the essential unity of nature" (*Darwin and Hegel*, p. 72). For Haeckel, this "essential unity" was a unity of substance, for the Idealists a unity of mind; but from a great many points of view their agreement is of more philosophical significance than their disagreement. And each called upon Darwin in support of his case.

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[27] As comes out especially in his *Ethical Studies* (1876). Metaphysics, according to Bradley, is not concerned to criticise the use of mechanical explanation within science – "this question of the operation of Ends in nature is one which in my judgment, metaphysics should leave untouched" (*Appearance and Reality*, p. 497) – but only the claims of science to describe "Reality."

[28] Especially in his first book, *The Riddle of the Sphinx* (1891).

Irving Goldman

EVOLUTION AND ANTHROPOLOGY

ANTHROPOLOGY HAS from its beginnings been closely identified with the concept of evolution. It was in response to the idea of evolution that anthropology developed its subject matter, the study of primitive and of prehistoric peoples, and acquired the orientation of an historical discipline concerned with origins and developments. Moreover, the Darwinian contribution raised the scientific standing of evolutionary theory and thereby invigorated a discipline already evolutionist in orientation. After 1859 professional field work was begun and anthropology passed into its maturity.

Maturity, however, also led to disenchantment. Hardly more than a generation after the *Origin of Species* came the reaction against theories of social and cultural evolution. Today, the question is asked whether there is room at all for an evolutionary school among the ramified interests of modern anthropology. What is perhaps the common view on this question was stated by the late S. F. Nadel, a leading British social anthropologist:

> The truth of the matter is that evolution belongs to those all-embracing concepts which, though inescapable, are too remote from the concrete problems of empirical enquiry to be much use in solving them. We need the concept of evolution, as it were, to satisfy our philosophical conscience; but the "laws" of evolution are of too huge a scale to help us in understanding the behaviour of the Toms, Dicks and Harrys among societies and cultures, which after all, is our main concern. Perhaps, indeed, there are not particular "laws" of evolution but only one law or postulate – if you like – that there *is* evolution.[1]

[1] S. F. Nadel, *The Foundations of Social Anthropology* (Glencoe, Ill., 1953), p. 106.

In view of the unspectacular progress of evolutionary theory in the English-speaking world, Nadel's judgment – though surely unwise – is not altogether unwarranted. Witness the comment of the late V. Gordon Childe, pre-eminent in evolutionary theory: "the last fifty years have not witnessed a refinement of evolutionary theory in anthropology but rather increasingly bitter criticism of the entire position."[2]

This was in 1949. Recently interest in cultural evolution has revived and criticism of the evolutionary position has softened. Since anthropology at no time seriously challenged the notions of economic stages and since anthropologists generally have held to a materialist conception of history – the keystone of nineteenth-century evolutionism in anthropology – there are no ideological obstacles whatsoever to a renascence of evolutionary thought and approaches in the discipline. The issue, indeed, is not criticism but neglect, not opposition but inertia on the part of evolutionists, who have been content to repeat the generalities that may have been exciting before 1900 but are now a little stale. But even this is changing. Today there are hopeful signs of fresh thought and greater concreteness in evolutionary theory.[3]

I

A review of evolutionary theory in anthropology since Darwin may tell us where theory went wrong and how it might be corrected. We may begin with E. B. Tylor (1832-1917) who has been called the "father of anthropology in all its British development."[4] Tylor, the amiable Quaker who lectured in anthropology at Oxford, offered no systematic theory as did the sociologists Comte and Spencer. What he mainly presented, and presented very well, were evolutionary perspectives. He examined varied primitive customs, showing their continuity with the present as "survivals" and their ubiquitous character in the primitive world as evidence of a common primitive mentality, and, where evidence seemed reasonable, suggesting historical antecedents. But Tylor was diffident about offering grand evolutionary conceptions, although, like his contemporaries, he spoke freely of "laws" and of "uniform stages" of development. In the course of later criticism, when

[2] V. Gordon Childe, *Social Evolution* (New York, 1951), p. 4.
[3] I am referring particularly to papers given at anthropological sessions and symposia on cultural evolution at meetings of the American Association for the Advancement of Science and the American Anthropological Association in recent years.
[4] "E. B. Tylor" by R. R. Marett in *Encyclopedia of Social Sciences* (New York, 1937).

evolutionists were excoriated for the offense of "unilinealism" (the belief that all societies moved upward through the same stages) Tylor came off well, not because he was free of the taint, but because he had made no major issue of it. His interest was in tracing customs back, rather than in setting them at starting points and watching them run the same course to the finish line. Tylor was an economic determinist of sorts as well, but this doctrine, too, rested lightly upon him.

Tylor's unusual qualities – his moderateness, his penchant for understatement, his disarming choice of "trivial" illustrations for a major point, and his sober and discerning judgment – insinuated the evolutionary position into anthropology without provoking sharp disagreement. At the height of anti-evolutionism, the reputation of Tylor was sufficient to preserve a beachhead for evolutionary theory, while his evolutionism was in itself slender enough and flexible enough to bend before criticism. By the same token, it had not the theoretical vigor to push forward.

Tylor's weakness was that he was a student of custom, not of society.[5] This was a major error from which evolutionary theory was slow to recover. It led to a "shreds and patches" view of culture history and encouraged the arbitrary assemblage of odds and ends of observations into logical but not necessarily historical sequences. This was the so-called "comparative method" of classical evolutionism. Most damaging of all, the method separated process – *how* evolutionary change takes place – from sequence, since only a natural history of societies reveals historical processes. Tylor's theory of animism, that a belief in spirits was the earliest form of religion, was developed systematically enough, but it was a Western rationalist view, not the view of a mind attuned to the quality of primitive societies.

Tylor's unfortunate choice of custom over society was not free but, as he explained, expedient. As late as 1871, the year his *Primitive Culture* appeared, there had been no professional field work among primitive peoples. The information that came from missionaries and other anthropological amateurs was incomplete even when accurate. There might well have been, as A. A. Goldenweiser once remarked, no evolutionary theory at all if nineteenth-century scholars had had to assimilate the ethnographic detail we now have.[6] Still, even a few full ethnographic studies would have given sharper perspective and a

[5] E. B. Tylor, *Primitive Culture*, Vol. I republished as *The Origins of Culture* (New York, 1958), ch. 1, esp. p. 8.
[6] *History, Psychology and Culture* (New York, 1933), p. 12.

greater awareness of context. The irony is that what was only an expedient for Tylor became methodological doctrine for those who followed him.

Yet even a natural history of customs gave promise of substantial development. Tylor himself took the next step of investigating what he called "adhesions," the consistent statistical associations among social customs. This method has since been vastly elaborated, notably by George P. Murdock at Yale. Tylor's work and reputation also added weight to the moral implications of evolutionary theory – the materialist conception of history, the continuity between so-called lower and civilized peoples, the panorama of ceaseless change and of human betterment, with European civilization as the crowning achievement of an awesome natural process.

Tylor's own contribution, nevertheless, was not tendentious. An anthropology of customs rather than of societies could have only a muffled social impact. Moreover, Tylor espoused "progress" modestly: "the collection of facts relating to various useful arts seems to justify the opinion that in such practical matters, at least, the history of mankind has been, on the whole, a history of progress."[7] Cautious as this statement was, "progress" came to be so firmly identified with evolution, that in many minds the evolutionary doctrine stood or fell on this issue. Undoubtedly, the need to stand on some firm ground with respect to so shifting and indefinable a concept as progress suggests at least one reason why technological explanations have been so appealing to evolutionists. The alternative is to drop the concept of progress altogether. This may be scientifically feasible, but in good conscience, the Science of Man cannot evade the issue either by ignoring progress or by limiting it to tools alone.

Herbert Spencer (1820-1903) stated the case for social evolution more fully than Tylor. Gifted at synthesis and with a flair for the memorable phrase ("survival of the fittest"), Spencer became the principal spokesman of so-called classical evolutionism. There is hardly a general observation in all evolutionary theory which cannot be credited to Spencer. Holding to an organismic analogy between biological forms and society, Spencer was able to present a coherent and logical analysis of social development. "Social phenomena," he declared, "form the subject matter of a science reducible, in some measure, at least, to the deductive form."[8] His deductions covered the entire range of social

[7] *Researches in the Early History of Mankind* (1865), p. 363.
[8] *Synthetic Philosophy: The Principles of Sociology* (New York, 1925), I, 597.

phenomena – economy, government, family, customs, religion, and thought. He dealt with customs and with social systems. His logical observations about social structures form the bases of contemporary social anthropology. Since the deductions referred to no particular societies but to Society, they are logical rather than historical. Spencer drew fully upon what there was of anthropological literature, but mainly to illustrate the deduction. Thus, the method was essentially immune from self-contradiction. One must, nevertheless, concede the brilliance of deduction, and credit Spencer with a serious effort at portraying something of the complexity of social evolution. The main formula itself was simple: evolution is adaptation leading to "greater size, coherence, multiplicity, definiteness" (*Synthetic Philosophy*, I, 597). Societies like organisms adapt themselves to their physical environments. Spencer regarded ecological adaptation as a primary source of social variation, recognizing, at the same time, its interaction with other conditions. But even though he acknowledged the complexity of social phenomena, he believed the general directions from ancestor worship to monotheism, from promiscuity to monogamy, from predatory society to industrial society, from primitive impulsiveness to civilized self-control and rationality were fixed by universal laws.

II

The dominant figure in nineteenth-century evolutionism was not Tylor but Lewis Henry Morgan (1818-81), the American attorney, banker, railroad magnate, and anthropologist. Morgan departed in some degree both from the Tylorean natural history of custom as well as from the Spencerian logical constructions and sought instead a natural history of societies. Morgan's theories had two good empirical anchorages. One was his first-hand knowledge of the Iroquois, evident in his book on *The League of the Iroquois*, which has been rightly called the "first scientific study of an American Indian tribe."[9] Direct ethnographic experience gave him the perspective of a whole society and of the special role of kinship in primitive society. Comparative studies of kinship systems provided the second empirical anchorage for Morgan's evolutionary position. Regarding the family as the pivot of social evolution, and the matrilineal clan as its earliest discoverable stage,

[9] Betty Meggers, "Recent Trends in American Ethnology," *American Anthropologist*, XLVIII (1946), 176-197.

Morgan began, with the help of the State Department, a world-wide study of "systems of consanguinity and of affinity" with the intent of classifying the systems and determining their historical relationships.[10]

Morgan used his information on kinship ingeniously, if not always soundly. He believed that kinship systems were adaptations to forms of marriage. Since terminologies change more slowly than practices, a "fossilized" kinship terminology, one that is no longer in harmony with the prevailing forms of marriage and of family, should reveal what the previous forms had been. The thesis failed to take into account the possibility that kinship might be an adaptation to conditions other than marriage and so fell into serious error. Still, Morgan had made his point as to the functional unity of a social system and had established a number of fundamental types of family, kinship, and marriage.

Morgan's "grand design," to define the social and cultural characteristics of economic stages, was most ambitious. He assigned peoples to appropriate levels within a sevenfold division: lower, middle, and upper Savagery; lower, middle, and upper Barbarism; and Civilization. Barbarism began with farming and Civilization with writing. Peoples were accorded their place in this hierarchy if they satisfied some diagnostic criterion such as possession of the bow and arrow, or knowledge of pottery or metallurgy. The arrangement worked well enough in many cases and was arbitrary and absurd in many others. Thus Hawaiian society, which by all other criteria would rank as a "high" culture, ranked low on Morgan's scale. For not only did the Hawaiians lack pottery and the bow and arrow, but they had a form of kinship system that Morgan had mistakenly interpreted as having emerged from the group marriage of brothers and sisters. The error was in the classical tradition of ignoring society as a whole.

The "grand design," with its elaborate attempt at synthesis, was not too successful. Morgan's real contribution was in the theory of social organization. As one would expect from a pioneer effort dealing with a body of material that was as unfamiliar and as technically forbidding as kinship systems, the results were in some respects crude, but they laid the ground for further research and for a sound theory of social organization. Morgan's recognition that the central issue in evolution is to account for the transformation of kinship-organized societies into territorial or political entities presented a research lead that is still fruitful. On this problem, Morgan's approach was historical, making use of

[10] *Systems of Consanguinity and Affinity* (1871).

the histories of the classical civilizations, the Greeks, the Romans, the Hebrews, and the Germanic tribes for direct evidences of transformation.

Morgan's historical materialism and his moral conviction that the future of society could not forever be bound up with the institution of private property suited the Marxian doctrine so well that Engels was able to incorporate *Ancient Society* (1877) almost fully into his own *Origin of the Family, Private Property and the State* (1884). Engels credited Morgan with having "discovered afresh in America the materialist conception of history" and ranked his work with that of Darwin and Karl Marx.[11]

The Marxist analysis moved more fully into the origins of the State. Engels portrayed a series of transformations of tool-making and subsistence leading to increased productivity, and of changes in systems of property relations that conformed to changes in productivity. Each technical advance provoked adaptive changes in the social organization and these, in turn, either promoted or held back productivity. In the Marxian system, economic productivity is the driving force of evolution. Production, insisted Marx and Engels, must and often does break through all man-made limitations, whether of a social or a technical kind. Thus, social change is not simple growth but a consequence of the opposition of forces inherent in the society itself and in the relationship between a society and its economic environment. In the Marxist view, an important distinction is to be drawn between patterns of change in primitive societies and patterns of change in later societies where social classes had already developed. In the former instance, the conflict is between Man and Nature, so to speak, and the primary issue is technology and resources; in the latter, the conflict is between Man and Man, and the primary issue is social, although tools and resource still play their part.

Increased productivity, as Engels saw it, provoked greed so that the newly created surpluses from farming and herding slipped from the control of the clan and into private hands. This momentous event disrupted social unity and led to the breakdown of the primitive clan organization and to the rise of the State. In the sequence of transformations leading to the State, Engels grants much to technology, but what he brings out, in the main, is a human drama of conflict for power and for position and of the antagonisms this provokes – a dramatic contrast

[11] Preface to 1st ed. (1884), *Origin of the Family* (New York, 1942).

to the simpler rationalist theory that political systems are administrative adaptations to "complexity."

For his own part, Engels over-idealized and even oversimplified the primitive world, obscuring its continuities with civilization. Thus he was led to regard the appearance of greed and interest in private property almost as psychological and social mutations. This was an unwarranted assumption. For the same reason, he failed to account for growth in economic productivity, assuming merely, that technology automatically led to increased productivity. What seems rather closer to the truth is the fact that those social and psychological factors which fostered economic productivity were, at the same time, instrumental in turning wealth and resources into private property. In the primitive world as in ours the possession of property enhances social status and provides the material bases for leadership and for power. Hence motives of status rivalry, conflicts for power, and social pressures upon leadership stimulate economic productivity and, at the same time, tend to concentrate goods and resources in the hands of chiefs and a social elite.

The weakness of Marxian theory is that it had no information and was therefore unable to account concretely for the whole sequence of events leading to stratification. The theory is at its best in dealing with post-stratified society, but, needless to say, its effectiveness as an analysis of class is diminished by the gap in accounting for the emergence of classes. Nevertheless, Marxism brought evolutionary theory to its intellectual high point. In the English-speaking world, however, the theory received little further development at the hands of anthropologists, partly for political reasons; anti-Marxists ignored it and pro-Marxists treated it too respectfully. When it *was* adopted it was usually reduced to a simple and mechanical economic determinism.

A different strand was added to evolutionary theory by Freud's *Totem and Taboo*. Although published in 1913, the book is nineteenth-century in its anthropology and stands as a link between the old and the new in evolutionary thought: old in its sketchy understanding of primitive societies, and new in its implications for a more complex view of human nature and of culture. The Freudian theory of ambivalence and its bearing upon the understanding of symbolic behavior is a particularly important contribution that has yet to be incorporated into evolutionary theory. Evolutionary theory has always been inclined to project a curiously flat and zoological human image; by forcing a consideration of the symbolic and nonrational elements in human nature, Freud made

his distinctive contribution to evolutionary theory. The rest of the Freudian theses are of doubtful value.

III

Tylor, Spencer, Morgan, Engels, and Freud represent the highlights but not the whole of nineteenth-century evolutionary thought in anthropology. The list of evolutionists is impressive: Sir Henry Maine on the evolution of law; Bachofen, McClellan, and Westermarck on the evolution of the family; Frazer and Durkheim on religion; Haddon and Grosse on art, and many others. With all their inadequacies, so readily seen from the vantage point of another era, these men posed the large questions, improvised methods of comparative study, and so provided frames of reference for more refined and more detailed studies.

By any objective evaluation that allows for the spirited crudities of the pioneer, these Victorians accomplished a great deal. They did succeed in establishing the concept of evolution by demonstrating historical unities among societies and by reasoning, if only on the logical grounds of limited possibilities, that some stages must have preceded others – stone before metal, hunting before farming, the clan organization before the State. The argument of succeeding stages may have fallen down in many specific cases, but it was strong enough on so many others that critics could not justifiably attack the general evolutionary concept but only its specific historical shortcomings. Acceptance of stages normally carries with it, as has been pointed out by Professor Leslie White, acknowledgment of progress as well.[12]

But the early vigor of evolutionary theory cannot be attributed to ethnology alone. If examples from living primitive societies seemed to offer reasonable evidences of stages, it was largely because the evolutionary process had been demonstrated so convincingly in biology. From within anthropology, the "hard" evidence came from archeology. Stratigraphy clearly revealed economic and technological as well as some evidences of social and religious stages. Among archeologists the most influential for evolutionary theory was V. Gordon Childe, who skillfully interpreted European and Near Eastern archeology along Marxist lines. Childe's studies[13] became models for interpretative arche-

[12] Leslie White, "Evolutionary Stages, Progress and the Evolution of Cultures," *Southwestern Journal of Anthropology*, III (1947), 165-192.

[13] Particularly his most popular books, *Man Makes Himself* (New York, 1936), and *What Happened in History* (New York, 1941).

ology and demonstrated the close interdependence of archeology, ethnology, and evolutionary theory. This interdependence, however, had its pitfalls. For example, interpretations that were tentative hypotheses in ethnology took on commanding authority after they had been applied to archeological data, so that archeology and ethnology often merely corroborated each other's biases. The bias of archeology is almost inevitably that of technological determinism, and as a result the work of Childe and others of the same school helped re-enforce a technological interpretation already strong in classical evolutionism.

On the doubtful side, classical evolutionism left a heritage of assumptions about human nature, society, and methodology that call for critical re-evaluation. On human nature, the primary assumption is the well-known doctrine of "psychic unity," namely, that human nature is a constant that responds in similar fashion to similar conditions. The concept cannot be "proved," but is necessary if we are to believe that we can understand other cultures, and that cross-cultural comparisons are meaningful. The issue, however, is not with the basic concept, but with its use. For classical evolutionists, psychic unity meant that if we understand how something happened in one society we can understand a parallel situation in another society even in the absence of independent evidence. In this way archeological findings can be interpreted from general knowledge of ethnography.

The assumption of a common human nature that follows from the concept of psychic unity is perfectly serviceable for a general and essentially abstract theory of cultural evolution, but is altogether too general to be of specific value to an historical approach. To an understanding of evolution in terms of the histories of specific societies, the variables of human nature are no less important than the constants. By variables I mean the values, motivations, needs, systems of thought and of symbolization that characterize specific types of societies. For all practical purposes, the classical evolutionists ignored these variables even though they all recognized an evolution of mentality from known primitive societies to civilization. But whether they believed with Marx and Engels that the primitive world had motives that were distinct from those of later civilization, or with Spencer, Tylor, Freud, and the entire French school from Comte through Lévy-Bruhl and Durkheim in a "primitive mentality," they nevertheless agreed in regarding the primitive world as a psychological constant. This we now know is an oversimplification.

The human image portrayed by classical evolution was that of Rational Man inventing out of necessity and ruled by basic "needs."

Such a view provided what at the time was necessary to demonstrate, that the laws of adaptive variation applied to the organic world as a whole, a parallel between cultural and zoological evolution. The difference between man and other animals was located in the human possession of a culture generalized as an instrument of adaptation, with its focus in tools. The popular works described man as the "tool-maker." No one was unaware that culture added a magnitude of complexity to human social evolution, but the full implications of such complexity were never taken too seriously. As long as the aim was mainly to establish the evolutionary perspective rather than to understand cultural variability in detail, the simple zoological portrait sufficed. For an historically-oriented evolutionism, needless to say, a more full-bodied human image is needed, in which the rational and the non-rational, the adaptive and the mal-adaptive are considered in a context of specific cultural systems. The classical picture of evolution also presented man in a closed deterministic system, driven by "given" needs and shaped by environment and tools. The system seemed closed only because the perspective was so distant. The historical perspective, that is, a close-up perspective, reveals variety, complex interactions, ranges of alternatives, and, therefore, a more "open" system of determinism.

Another feature of the classical view was that of fixed laws and fixed uniformities of change. Strongest on this score was the Marxist conviction. But even so moderate a spokesman as Tylor argued that "Human institutions like stratified rocks succeed each other in series substantially uniform over the Globe, independent of what seem the comparatively superficial differences of race and language, but shaped by similar human nature."[14] This assumption of unilineal evolution became the prime target of criticism, and it was an easy target since a single negative example destroyed the thesis. Perhaps too much has been made of the issue of unilinealism. Classical evolutionism sought regularities, as it should have, and, according to its assumptions about human nature and culture, the broad perspective was surely uniform. Whether *particular* stages follow one another in uniform series is an empirical question, however, and some stages require a specific antecedent while others do not. Stone tools must precede metals, but matriliny does not have to precede patriliny. Growths in differentiation, growths in complexity, or the transformation of "kinship" societies into "territorial" societies represent general evolutionary features that do not perhaps follow rigid laws but are common enough to merit recognition as uni-

[14] Cited in Childe, *Social Evolution*, p. 5.

versal trends. The classical evolutionists were in error on empirical rather than upon fundamental theoretical grounds, and there is no need to counter the doctrine of unilinearity with an opposite doctrine of multilinearity as some evolutionists are now doing.

The weakness of a method that had to proceed deductively from a priori positions to use data only illustratively rather than analytically was inevitable in view of the undeveloped state of archeology and of ethnology. The criticism, so devastatingly made by the Boas school, that the comparative method of the classical evolutionists could demonstrate the evolutionary thesis merely by a proper choice of examples was only too telling. Tylor, to be sure, tried to control his data by statistical correlation,[15] and Morgan drew upon histories wherever available. But there were no suitable means for reconstructing the histories of preliterate societies until the ethnographic and archeological records had become reasonably complete. Only now is historical reconstruction a feasible and proper task for anthropology.

Compelled to deal with an incomplete record, nineteenth-century evolutionists followed the example, by analogy, of paleontology, a discipline that also works with the handicap of an incomplete record. The major stages of Savagery and of Barbarism were taken to be strata within which customs occupied a reasonably predictable order. From zoology was drawn the concept of "survivals" to describe features that seemingly had been carried upward in recognizable form from a lower stage. Tylor regarded survivals as most important evidences of historical continuity, a wedding that was ceremonialized by a violent handling of the bride being interpreted as a vestige of an earlier system of marriage by capture. Without specific historical evidence, however, one cannot be certain of this interpretation, any more than one could be certain that a social custom "belonged" to a particular stratum in an evolutionary series.

IV

It was, therefore, on the grounds that its specific avowals failed to meet the test of actual histories that evolutionism encountered its strongest criticism. The critique of the evolutionary position began well before the close of the nineteenth century and continued well past the

[15] "On a Method of Investigating the Development of Institutions," *Journal, Royal Anthropological Institute*, XVIII (1889), 245-269.

nineteen-twenties, wearing itself out only after rebuttal had ceased. The attack came on three fronts — from the so-called Diffusionist, Functionalist, and Historical schools — and struck not at the concept of evolution as such but at the evidence. The diffusionist critique forced a consideration of history. If cultural and social features move across the world and are borrowed by simpler peoples from more advanced peoples, then, of course, sequences as well as processes will not be uniform, argue as one may that a society will borrow only what it is prepared to accept. Diffusionists such as Elliott Smith[16] in England and Father Schmidt[17] and his Austrian clerical co-workers were not anti-evolutionists. They believed that evolution occurred in a few centers and then spread outward. Their interest, however, was in the diffusion from the centers and not in the evolutionary developments that may have taken place there. Diffusion is, of course, a basic historical process, but diffusionism as a school is only quasi-historical in that its focus is custom, not society, and it can therefore offer no searching analysis of cultural development other than to point to sources of cultural influence.

Functionalism is identified with the British school of Bronislaw Malinowski.[18] Offering a physiological approach to society, functionalism is not historical in that it disregards both culture change and the specific configurations of cultures. Rather, in the spirit of classical evolutionism, it regards culture as an adaptive system for satisfying "basic needs." But where the early evolutionists saw transitions and change, functionalists observe only function. A society to the functionalist is an organism seeking to maintain an equilibrium with respect to needs for food, procreation, health, shelter, defense. Basic needs in turn create secondary needs such as government, education, and religion. Functionalist analysis is therefore instrumental, attempting to demonstrate how each aspect of a system functions to maintain or to restore equilibrium. Evolutionary change is regarded as external to and not inherent in a social system and can therefore be ignored. After all, the functionalists may say, the physiology of the liver is independent of time and place. The kinship of functionalism to Spencer is apparent. It is Spencerian, however, without the Spencerian interest in sequences.

Functionalism was not a direct attack upon evolutionary theory. It offered rather a refuge from the loose historical reconstructions of the evolutionists and of the diffusionists and so moved many anthropologists

[16] *In the Beginning: The Origin of Civilization* (New York, 1928).
[17] *The Cultural Historical Method of Ethnology* (New York, 1939).
[18] "Culture" in *Encyclopedia of Social Sciences*, and *A Scientific Theory of Culture and Other Essays* (Chapel Hill, 1944).

out of history into sociology, where British social anthropology is today. This refuge has proved to be of limited value since, as a physiology by analogy only, functionalism verges on triteness. From this dead end it was relieved by A. A. Radcliffe-Brown who turned functionalism to the study of social structure.[19] Structuralism, as this approach is sometimes called, has its roots in Spencer as well as in Morgan. Its concern is with delineating types of social systems, their characteristics, and their patterns of variation, as in Radcliffe-Brown's study of Australian kinship systems.[20] Although its findings are of value to evolutionary theory, inasmuch as it is the social structure that controls the patterns of evolutionary change, structuralism is not an evolutionary school. Structural analysis should be able to answer the evolutionary question put by Spencer: how do complex structures emerge from simpler structures? This question, however, is not the heart of structuralist interest. A specialized school, it has generally refrained from historical reconstruction which Radcliffe-Brown has derided as "pseudo-history." It has also excluded from consideration motives, values, attitudes, and ideologies, in short, the core of what is commonly regarded as culture. Concentration upon a limited objective has, on the other hand, added depth to the analysis of structure, and when the synthesis between the sociological, the historical, and the cultural is made, the value of structuralism to evolutionary theory is enhanced.

Whereas British anthropology reacted against historical reconstruction by turning to comparative sociology, American anthropology, under the influence of Franz Boas, reacted by demanding more precise history. Boas deplored the scientific status attributed to evolutionary theory, but he was no anti-evolutionist. In his Presidential Address to the American Association for the Advancement of Science in 1932 he defined the aims of anthropological research as follows:

> We may perhaps best define our objective as the attempt to understand the steps by which man has come to be what he is, biologically, psychologically and culturally. Thus, it appears at once that our material must necessarily be historical material, historical in the widest sense of the term. It must include the history of the development of the bodily form of man, his physiological functions, mind and culture. We need a knowledge of the chronological succession of forms and an insight into the conditions under which changes occur. Without such data progress seems impossible and the fundamental question arises as to how such data can be obtained.[21]

[19] *Structure and Function in Primitive Society* (London, 1952).
[20] "Social Organization of Australian Tribes," *Oceania Monographs* (Sydney), I (1930).
[21] *Race, Language and Culture* (New York, 1940), p. 244.

This forthright statement of an evolutionary position did not make an evolutionist of Boas since he did not undertake evolutionary studies of his own, nor did he encourage his students to do so. In the final analysis the test of a position is its practice. Boas did not believe that anthropology was ready for scientific evolutionary theory. Bound by an ironclad empiricism, and a reluctance to speculate, he also had a deep distaste for bombast and for the trite. He could not accept current deterministic stereotypes, economic, geographic, or psychological. Logical arguments for evolution, such as that stone preceded metal, or that hunters with poor terrain cannot form large settled communities, he justly regarded as too puerile to discuss. In the end, Boas became pessimistic at prospects of arriving at "general laws," the supreme aim of classical evolutionism. He urged, instead, that anthropology concern itself with the study of more limited problems.

Boas undertook, then, to fill in the historic record of primitive societies by amassing detail, and above all, by seeking to allow the record to speak for itself. Classical evolutionism had come with much ready-made theory for which it sought out the "evidence." Boas restored historical sanity by insisting that we listen to the native. His work among Indian tribes of the North Pacific coast assembled an unparalleled collection of information in native texts.[22] Anthropology and evolutionary theory have yet to assimilate all this detail.

If Boas demanded historicity and complexity, he also offered an approach for achieving it. He urged the intensive study of limited geographic or culture areas, suggesting that the systematic comparison of one area with another might yield valid and significant generalizations. His essay on the "Limitations of the Comparative Method" in which he made this proposal was published as early as 1896[23] and has been one of the most influential documents in American anthropology – except for those evolutionists who apparently feared that to stray from the direct search for universal stages was to abandon evolutionary theory entirely.

Boas' distaste for systems, and his insistence that there was never enough information upon which to generalize may not have immediately hastened the growth of evolutionary theory. Some critics have even charged that Boas held it back. This is unjust. The Boasian approach prepared the way for later developments in evolutionary theory

[22] His writings on the Kwakiutl and Tsimshian Indians, much of it still unpublished.
[23] *Race, Language and Culture*, pp. 270-280.

by creating the necessary empirical bases and by fostering diversified approaches to ethnology. If contemporary ethnology is now rich in implications for evolutionary theory, it has its critics, such as Boas, to thank more than its friends. If evolutionists have been slow to draw upon this information, it is because their thought is still largely in the nineteenth century and their tradition has become more polemical than problem-minded.

The leading exponent of classical evolutionism in America is Professor Leslie White of the University of Michigan.[24] White's interests are in the "grand design," the general stages and the general processes of cultural development. Temperamentally the opposite of Boas, White sees details and historicity as obscuring the main picture. While White's evolutionary views are not original, they have a classic simplicity. To him culture is an instrument for harnessing energy. The plan of evolution, accordingly, is encompassed by the stages from hurling a stone to unleashing atomic power. Unlike Morgan and Engels, for whom evolution was, at least in part, the histories of societies, White is interested in culture in general, and seeking the single principle, he finds no need for ethnographic detail except, as with Tylor and Spencer, for convenient illustrations. So general a thesis is necessarily logical and indisputable. Inconsistencies and contradictions, if they can arise at all, are ruled out as mere details, while histories that fit the logic are called to the witness stand. White's thesis of technological determinism needs no historical evidence at all since it is unarguable that a society using the digging stick will be different from one using tractors. Since only functionalism is adapted to a generalized theory of culture, White's evolutionary theory is necessarily functionalist.

A more sophisticated and more significant approach to evolution is that of Professor George P. Murdock at Yale, even though Murdock, in the Tylorean tradition, deals with the natural history of customs rather than with societies.[25] Murdock has chosen empirical problems in social structure, not to establish a single scheme of evolution of kinship and of family but, like Morgan, to identify varieties of social structure and their modes of variability. Drawing upon information from some 250 societies Murdock has ruled out, from the start, simple linearities of evolution. He has replaced schematic determinisms with probability statements and generalized social systems with more specific types and

[24] In particular *The Science of Culture* (New York, 1949) and *The Evolution of Culture* (New York, 1959).
[25] "Evolution and Process," in A. L. Kroeber, ed., *Anthropology Today* (Chicago, 1953).

subtypes. Finally, he has offered data in the form of carefully stated and, therefore, testable propositions and hypotheses. In many respects a descendant of the classical school, Murdock has brought close to fruition the intentions of Tylor and Morgan with respect to discovering the evolution of social systems. Murdock's focus upon the natural history of customs, however, stands as a serious limitation upon the significance of his work. His social structures are not historical entities, but statistical abstractions. We still need to know to what extent these abstractions conform to historical reality. Nor is there any certainty that social structure is as independent of culture as Murdock and structuralists generally assume it to be. Murdock's propositions will have to be matched against specific historical situations.

Professor Julian Steward of the University of Illinois defines evolution as the "discovery of laws of parallel development."[26] He has decided rather arbitrarily and inexplicably that while the genetic relationship between forms is the essential evolutionary principle in biology, the very opposite – that is, parallels in sequence among historically *unrelated* societies – is to be regarded as the test of evolution of society. One suspects here a confusion between evolution and one of the aims of evolutionary theory. The confusion is compounded when Steward disclaims unilinear evolution in favor of a "multilinear" position which, since it follows an historical approach in dealing with details and with special cases, is hardly appropriate to Steward's own definition of evolution as "scientific and generalizing" rather than historical. Those of Steward's empirical studies with important evolutionary implications, such as his papers on patrilineal bands and on the theory of social adaptation to ecology, draw upon specific culture areas and are definitely historical.[27] In the main, however, Steward is searching for uniformities of processes of evolutionary change rather than for uniformities of stages, and because his frame of reference is so broad, his processes turn out to be the familiar ecological and technological determinisms of classical evolutionism popularized by the late V. Gordon Childe.

The difficulty in Steward's evolutionary position is that he has been unable to reconcile a strong historical bent with his arbitrary definition of evolution as "general laws." In this, he reflects an inherent difficulty in evolutionary theory, that of reconciling history and science without over-diluting history and over-generalizing science.

[26] *Theory of Culture Change* (Urbana, Ill., 1955), esp. Pt. III.
[27] *Race, Language and Culture*, pp. 270-280.

V

Granting the always present difficulty of reconciling history and science in any approach, the starting point of evolutionary theory must be history.[28] If evolutionary theory is to be historical – and no good history, as Kroeber reminded White,[29] is devoid of general theory – it must be defined in the strictly Darwinian sense of descent with modification. Specifically, cultural evolution is to be regarded as a branch of history, concerned with cultural variations leading to new and relatively stable forms. Such a definition encompasses small-scale as well as large-scale modifications, a grand history of human societies as well as of sequences of forms of a single society. And if evolution is to acknowledge context, it must be based upon the history of societies rather than upon the history of customs. There are many types of evolution and many levels of cultural evolution, and all types and all levels are pertinent to a general evolutionary theory, but their scientific interest does not depend upon universality. A unique transformation is also a part of evolution and is no less significant because of its uniqueness. The aims of evolutionary theory are to discover the ways in which new and relatively stable forms arise from preceding forms, their successive sequences, and their implications for cultural progress. The "grand design" of cultural evolution, the sweep from the lower Paleolithic to the high point of industrial society, must remain general and largely conjectural since so much of the historical record is irrevocably lost, but there is ample evidence for general theories of cultural evolution drawn from concrete studies and from a comparison of stages and processes that are revealed by culture area studies. Even today, known culture areas encompass a scale from the simplest hunters and gatherers through almost every variety of farming community.

The question is, then, what histories of prehistoric or of primitive societies do we, in fact, know in detail? Are we not attempting to be historians without historical documents or direct records? Archeology, to be sure, tells much about societies that are rich in durable artifacts, but the archeological reconstruction is necessarily incomplete and one-sided, virtually imposing a technological and ecological interpretation. If archeological interpretations are to be amplified, they must draw

[28] See in particular Alexander Lesser, "Evolution in Social Anthropology," *Southwestern Journal of Anthropology*, VIII (1952), 134-146; Childe, *Social Evolution*, p. 1; and A. L. Kroeber and Clyde Kluckhohn, *Culture: A Critical Review of Concepts and Definitions*, Peabody Museum Papers, XLVII (Cambridge, Mass., 1952).

[29] "History and Evolution," *Southwestern Journal of Anthropology*, II (1946), 1-15.

upon ethnological theory. Thus, the source of evolutionary theory is inescapably in ethnology, since it is ethnology that must devise methods of historical reconstruction that will offset the disability of lack of historical records. Comparative studies in ethnology are forms of historical reconstruction because they reveal probable associations of customs. Tylor, it will be recalled, was the first to apply a method of statistical correlations of cultural traits. But world-wide comparative studies, no matter how elaborate, have the defect of obscuring all but the most general meanings. Since evolutionary theory foundered on generalities, its development can only be set moving again not by new generalities but by more concreteness. The need for close analysis that will bring out new relationships and so deepen and refine evolutionary theory takes us back to the viewpoint of Boas that the center of inquiry must be a limited geographic area. In modern terms this would be a "culture area," that is, a geographic grouping of societies that share some historic unity, whether a unity of descent as demonstrated by common language or a unity of common social systems. In either case, the evolutionary problem is to explain variations of underlying patterns within the area, a variation in pattern being taken to be an illustration of the developmental characteristics of a particular social system.

The culture area concept was a contribution of American anthropology offered originally as a taxonomic device for museum exhibits. Clark Wissler, Kroeber, and others, gave it theoretical content by showing that the cultural groupings of American Indian tribes correlated, in general, with ecological areas.[30] It was also evident that no culture area was homogeneous. Variability was disturbing to the taxonomist, but not to the historian, since it is the variability of the component societies of an area that provides the basis for a reconstruction of their histories. The degree to which historical reconstruction can be "controlled" within an area has been demonstrated by several important studies.[31] The value of the culture area approach lies in its avoidance of the schematism and oversimplification of classical evolutionism and of its modern adherents, while it renders artificial the usual distinction between science as generalizing and history as particularizing. Since all comparisons are in

[30] Kroeber, *Cultural and Natural Areas of Native North America* (Berkeley, Calif., 1939) and Clark Wissler, *The Relation of Nature to Man in Aboriginal America* (New York, 1926).

[31] Examples are F. Eggan, *Social Organization of the Western Pueblos* (Chicago, 1950), S. F. Nadel, *The Nuba* (London, 1947), E. R. Leach, *Political Systems of Highland Burma* (London, 1953), M. Fortes and E. E. Evans-Pritchard, eds., *African Political Systems* (London, 1940).

context, generalizations do not violate historical relativism, and analytic depth is limited objectively only by the quality of the ethnographic data. Since whole cultures are considered, the culture concept is used fully, account being taken of integration, pattern, and ethos.

The primary technical problem in analyzing a culture area for purposes of historical reconstruction is to chart the patterning of variations. How this is to be done and what the technical difficulties involved will be will depend upon the scope of the problem and the character of the information available. In a study of Polynesia,[32] the writer sought to determine the most generalized patterns of culture and of institutions from the point of view of a focus. In this case, the focus was taken to be the system of regulating social status and its corresponding motives of status rivalry. Status rivalry is perhaps basic in all active evolutionary sequences leading to civilization. It is certainly basic in Polynesia where hierarchical status regulates the main social, political, economic, and religious activities. Status rivalry alters status systems and in this way affects indirectly all other regulatory centers. Thus, the culture area approach can provide a reasonably well-controlled comparative method for historical reconstruction, one that takes into account diffusion, inner processes of development, pattern, structure, and function. Dealing with cultures at any desired level of concreteness, it allows evolutionary theory to draw upon all of anthropology and related disciplines. Synthesis has always been a powerful stimulus to theory, and evolutionary theory is, above all, a synthetic theory.

Concreteness and synthesis should lead to the long overdue revision of current conceptions of adaptation. Adaptation theory is now strictly functionalist and rationalist. Yet, evidence is very strong that motivations, the focus of adaptation, are far more complex than allowed for in functionalist theory. Students of the so-called socialization process have, for example, drawn attention to subconscious sources of receptivity and resistance to change.[33] But there is no need to invoke psychoanalytic theory to deal with the role of motivation in evolutionary change; other and more directly understood motives – status and power, religion, social cooperation – interact with primary economic motives and are, therefore, of major importance. But, granted that economy is primary in evolution, it is still necessary to deal with economic factors fully and not merely as ecology or technology. An economy has its ap-

[32] Irving Goldman, "Status Rivalry and Cultural Evolution in Polynesia," *American Anthropologist*, LXVII (1955), 680-697.

[33] I. A. Hallowell, "Personality Structure and the Evolution of Man," *American Anthropologist*, LXII (1950), 159-173.

propriate social organization, and goods and resources have symbolic as well as subsistence functions.[34]

The concept of adaptation is linked to equilibrium theory. The conventional evolutionary view is that of a static equilibrium, that is to say, that unless disturbed by outer events, new inventions, or population pressure, a society will remain stable. There are, of course, societies with stable equilibrium, but there are also societies whose structures poise them for evolutionary change. The scientific problem is to identify the equilibrium and adaptation characteristics of particular social systems and not merely to assume some generalized patterns. Types of patterns and their bearing upon social evolution are more likely to be identified if whole societies in their context of culture area are studied.

In this survey of the Darwinian influence on anthropology, we have seen that classical evolutionism set forth the general outlines and perspectives of theory. It could not do more. In those instances where the older theory could move past the general into some measure of historical concreteness, it illuminated our understanding of evolution brightly, but where it remained with generalities, its light dimmed and we were left to peer at the poor shadows of men. Even so, the classical evolutionism of the leading Victorian anthropologists presented a rich spectacle that embraced the full range of human cultural life: economy, society, government, religion, art, myth, and speculative thought. Later in the nineteenth century evolutionary theory narrowed the stage but without giving more definite form to the shadows. The historical view that came into prominence early in this century, on the other hand, has usually been generous and vivid, fond of the details of human life, and yet capable of seeking out their general significances. The next step in evolutionary theory will surely be historical. The evolutionary theory will then find its substance in and have relevance to the Toms, Dicks, and Harrys of the human world. Thus we shall have returned to the Darwinian spirit in evolution, with its ceaseless curiosity about all the details of the changing forms of life.

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[34] One of the most valuable treatments of economics from an evolutionary point of view is Richard Thurnwald's *Economics in Primitive Communities* (London, 1932). Thurnwald has gone far beyond conventional economic determinism. Simple economic determinism has come under criticism in recent years from a number of anthropologists: cf. John W. Bennett, "The Interaction of Culture and Environment in the Smaller Societies," *American Anthropologist*, XLVI (1944), 461-478, and Henry Orenstein, "The Evolutionary Theory of V. Gordon Childe," *Southwestern Journal of Anthropology*, X (1954), 200-214.

Cyril Bibby

HUXLEY AND THE RECEPTION OF THE "ORIGIN"

THE MOST IMPORTANT BOOK of its century, Darwin's *Origin of Species,* catalysed a complete rearrangement of ideological patterns over a wide range of human thought. It is an interesting question why the book's impact was so immense. It was partly, no doubt, that its thesis bore so closely on vital matters of belief and speculation; partly the masterly manner in which vast numbers of facts were marshalled into overwhelming array; partly the deceptive blandness of style and simplicity of statement which allowed readers to imagine that they really understood the book. And yet theories of evolution were not new: they had cropped up repeatedly among the Greeks and Romans, and in more recent times had been proposed by both Lamarck and Darwin's own grandfather. Not even the theory of natural selection was entirely new: although Darwin was unaware of it, he had been in some measure anticipated by W. C. Wells in 1813 and in more detail by Patrick Matthew in 1831.[1] Why, then, since these earlier evolutionists had made comparatively little impression and been but little reviled, was Darwin at once so successful and so abused?

It is a fairly sound general rule that excessive indignation and unreasoning abuse arise from fear and insecurity, and, though the Eng-

[1] W. C. Wells, *Two Essays: One upon Single Vision with Two Eyes; the Other on Dew . . .* (1818); Patrick Matthew, appendix to *Naval Timber and Arboriculture* (1831).

land of 1859 was calm on the surface, on the level of ideology she was deeply apprehensive. The European revolutions from 1789 on were not forgotten; it was not many years since England had suffered widespread riots and arson; there had recently been the Eureka Stockade revolt in Australia and the Mutiny in India; Napoleon III was at war in Italy. In 1845 Newman had seceded to Rome and given the national church a severe fright; in 1846 the English translation of Strauss's *Leben Jesu* had brought a shock from another direction; soon the Christian Socialists were actively spreading novel views; in 1850 came the "Papal Aggression"; in 1853 F. D. Maurice's *Theological Essays*, which led to his ejection from his Chair at London's King's College, gave yet another jolt; in 1858 H. L. Mansel produced his defence of orthodoxy, *The Limits of Religious Thought*, one effect of which was to remind Huxley of the man in Hogarth's election scene, sawing away his opponent's inn sign without noticing that he himself was sitting on it. There was much to indicate the need for closing the ranks of conventional thought against subversive ideas.

This may explain the virulence of some of the opposition to Darwin, but it does not explain all the opposition. To many scientists, including some very eminent ones, the evidence in favour of the transmutation of species appeared inadequate to overturn the traditional (and still scientific) view of independent creation. And today, when one usually thinks of evolution in biological terms, it is well to remember that the very word "biology" was coined only in 1813 and that in this context "scientists" must very largely mean "naturalists." For geologists, in particular, the succession of strata in the rocks, each with its characteristic fossil content, was not implausibly explained as the result of a succession of cataclysms and subsequent creations, a view held even by the master-geologist, Cuvier himself. The Pentateuchal myth, arising among a people influenced by the culture of the recurrently flooded valleys of Tigris and Euphrates, could be presented quite credibly as a record (or folk-memory) of the most recent cataclysm. And, when someone as intelligent as Newman could assert that the value of Revelation was shown by the fact that without it we should never have been able to discover that Noah had preserved the animals in his ark, it is not surprising that the common view of creation was still that versed by Milton a couple of centuries earlier:

> The earth obey'd, and straight,
> Op'ning her fertile womb, teem'd at a birth
> Innumerous living creatures, perfect forms,
> Limb'd and full grown.

To the average believer, Darwin appeared to be not simply promulgating a new scientific theory, but destroying the foundations of belief; and, when a great comparative anatomist like Richard Owen asserted the theory to be untrue, it is not surprising that he and his like were greeted with open arms as scientific advisers to the Establishment.

It was fortunate for the Darwinians that Owen was a man too vain to think that he might be mistaken. Instead of admitting that Huxley was right and he was wrong in the debate about the degree of similarity between man and ape, Owen lost the opportunity to give way gracefully, and so made his position increasingly untenable by a stubborn refusal to face facts. When the fall came, it was in consequence the more resounding, and the *Punch*-reading public was treated to a series of cartoons and squibs reflecting the controversy. One, headed "The Gorilla's Dilemma," had this touching first stanza:

> Say am I a man and a brother,
> Or only an anthropoid ape?
> Your judgment, be't one way or t'other,
> *Do* put into positive shape.
> Must I humbly take rank as quadruman
> As OWEN maintains that I ought:
> Or rise into brotherhood human,
> As HUXLEY has flatt'ringly taught?
> (XLIII [18 Oct. 1862], 164)

Owen's defeat was reflected in another set of verses, headed "Monkeyana" and addressed from the Zoological Gardens over the signature of "Gorilla":

> Then HUXLEY and OWEN,
> With rivalry glowing,
> With pen and ink rush to the scratch;
> 'Tis Brain *versus* Brain,
> Till one of them's slain;
> By Jove! it will be a good match! . . .
>
> Next HUXLEY replies
> That OWEN he lies
> And garbles his Latin quotation;
> That his facts are not new,
> His mistakes not a few,
> Detrimental to his reputation.
>
> To twice slay the slain
> By dint of the Brain
> (Thus HUXLEY concludes his review),
> Is but labour in vain
> Unproductive of gain,
> And so I shall bid you "Adieu!"
> (XL [18 May 1861], 206)

For the lower orders, less likely to smile over *Punch* than to chortle over the vigorous burlesque pamphlets of the time, there was "A report of a SAD CASE Recently tried before the Lord Mayor, OWEN *versus* HUXLEY, In which will be found fully given the Merits of the great Recent BONE CASE" (1863), with page after page of this sort of thing:

> Policeman X — "Well, your Worship, Huxley called Owen a lying Orthognathous Brachycephalic Bimanous Pithecus; and Owen told him he was nothing but a thorough Archencephalic Primate."
> Lord Mayor — "Are you sure you heard this awful language?"

It is difficult today to understand the enormous interest taken by the public of the 1860's in science, when scholars and streetsweepers alike knew that Huxley had proved right and Owen wrong. As Huxley won battle after battle, he rapidly became science personified and the most influential scientist there has ever been in public affairs; his own immense personal prestige seemed like a guarantee that Darwin's theory was correct.

Yet, when powerful and varied ideological interests are defeated so quickly and so completely as in this case, there are likely to be contributory causes deep down below the surface appearance of things. It is true that Darwin's patient accumulation of evidence provided a much more powerful brief than any predecessor had prepared. (Huxley noted, "Mr. Darwin abhors mere speculation as nature abhors a vacuum. He is as greedy of cases and precedents as any constitutional lawyer."[2]), but not even the scientific world would have been so quickly convinced had it not been already in a sense awaiting conviction.

Below the public surface of geological orthodoxy, firmly based on catastrophism, there had never ceased to flow many private rivulets of doubt. On the Continent, from Fracastoro and Leonardo in the sixteenth century and Steno in the seventeenth to Desmarest in the eighteenth, there had been those who suspected that the past of the earth could be explained in terms of forces presently observable. In 1785 England saw the publication of James Hutton's *Theory of the Earth*, a splendidly argued exposition of uniformitarian geology. Hutton failed to make much impression, but meanwhile more canals were being cut and before long a great burst of railway construction made it easy for Englishmen to make geological observations at home, in addition to

[2] "The Darwinian Hypothesis," *Collected Essays* (1893), II, 20.

those made during increasingly popular European travel and on long sea cruises. When the first volume of Charles Lyell's *Principles of Geology* appeared in 1830, to be discounted by most of the geological mandarins, many of the younger men were immensely impressed by its argument that the earth had slowly evolved into its present form, and the second volume dealt with plant and animal life in a manner which somehow only just failed to assert its evolution. No doubt there were many who still believed, and more who affected to believe, that old John Lightfoot of Cambridge had been correct in asserting that "Man was created by the Trinity on 23rd October, 4004 B.C., at nine o'clock in the morning," but some were beginning to think in terms of millions and hundreds of millions of years rather than in thousands. Long before the *Origin* appeared, the impregnable rock of Scripture was suffering slow erosion, ultimately as effective as the erosion which the uniformitarian geologists postulated for the earthly rocks. Geological theory, despite its firm surface, was in a flux within, and the impact of Darwin's book was sufficient to crack it wide open.

Biology, too, new as it was, was already on the verge of a fundamental reassessment. Erasmus Darwin's *Zoönomia* (1794) made as little impression on most people as it did on the young Charles Darwin, "the proportion of speculation being so large to the facts given,"[3] and even Lamarck's much better based *Philosophie zoologique* (1809) was little known in England. Darwin, indeed, denied that men's minds were already prepared for his theory, asserting that he "never happened to come across a single one who seemed to doubt about the permanence of species," but he immediately followed this with the more significant remark that "innumerable well-observed facts were stored in the minds of naturalists, ready to take their proper places as soon as any theory which would receive them was sufficiently explained" (*Autobiography*, p. 124). Increasingly, with accumulating biological knowledge, the idea of independent creation had become less credible, and many, even outside the professional ranks of biology, were beginning to entertain the possibility of evolution.

This may help in part to explain the ready response which Darwin's book received among those who were not too deeply committed to orthodoxy. Even Sir Charles Lyell, "up to that time a pillar of the anti-transmutationists (who regarded him, ever afterwards, as Pallas

[3] *The Autobiography of Charles Darwin*, ed. Nora Barlow (London, 1958), p. 49.

Athene may have looked at Dian, after the Endymion affair),"[4] declared himself a Darwinian and was a tower of strength. As for the younger men, T. H. Huxley and Alfred Newton[5] were probably not the only ones to declare their vexation at not having thought out so simple an explanation of things for themselves. Far away in New Zealand Samuel Butler received the book as a flash of illumination; at home the first edition was sold out on the day of publication and the second soon after; in Europe translations rapidly appeared in many languages and the vast literature of "Darwinismus" soon called up regular German bibliographies. People were ready for an evolutionary explanation of things and natural selection provided a credible mechanism simple enough to be understood by scientist and layman alike.

The major battle between evolution and orthodoxy, highlighted by the 1860 Oxford clash between Professor Huxley and Bishop Wilberforce, has been often enough described. What is sometimes comparatively neglected is that there were many clergy quite ready to come to terms with the new scientific outlook. Even Newman, despite his conversion to Rome, saw no especial intellectual difficulty in accepting organic evolution (with, of course, certain theological reservations about the human soul), and in the Church of England there were men who accepted the idea from the start. This was a time when, as G. M. Young has remarked, "English society was poised on a double paradox . . . Its practical ideals were at odds with its religious professions, and its religious belief was at issue with its intelligence."[6] Charles Kingsley was one of those who were anxious to bring their religion into line with their intelligence and their practice into line with their religion – once, indeed, he commented to Grant Duff, in his curious stammer, "We shall never do any good, till we have got rid of those o–dious words Re–ligion and Christi–anity."[7]

Kingsley's views come out very clearly in his fascinating correspondence[8] with Huxley. The two had first met in 1855, and four years later Kingsley wrote to express appreciation of Huxley's review of Darwin's book in *Macmillan's*. In the following year, when Huxley suffered the death of his first child Noel, the clergyman sent him a letter of con-

[4] *Life and Letters of Charles Darwin*, ed. Francis Darwin, 3 vols. (1887), II, 231.
[5] Alfred Newton (1829-1907) was appointed first professor of zoology and comparative anatomy at Cambridge in 1866.
[6] "Portrait of an Age" in *Early Victorian England* (London, 1934), II, 426.
[7] Mountstuart E. Grant Duff, *Notes from a Diary, 1886-1888* (1900), I, 170.
[8] In the muniments of the Imperial College of Science and Technology.

solation which evoked from Huxley a baring of the soul quite exceptional for so self-controlled a man, and thereafter they were close friends. Kingsley had no qualms whatever about accepting Darwin's theory, and Huxley wrote "it is clear to me that if that great and powerful instrument for good or evil, the Church of England, is to be saved from being shivered into fragments by the advancing tide of science – an event I should be very sorry to witness, but which will infallibly occur if men like Samuel of Oxford are to have the guidance of her destinies – it must be by the efforts of men, who, like yourself, see your way to the combination of the practice of the Church with the spirit of science. . . . I don't profess to understand the logic of yourself, Maurice, and the rest of your school, but I have always said I would swear by your truthfulness, and sincerity, and that good must come of your efforts" (23 Sept. 1860).

Soon, indeed, it was Huxley who was the restraining influence, and by the spring of 1863 he was warning Kingsley that there was as yet no evidence that selection could produce specific sterility: "From the first time that I wrote about Darwin's book in the *Times* and in the *Westminster* until now, it has been obvious to me that this is the weak point of Darwin's doctrine. He *has* shown that selective breeding is a *vera causa* for morphological species; he has not yet shown it a *vera causa* for physiological species" (30 Apr. 1863). It was in this year that Kingsley's satire *The Water Babies* appeared, with its suggestion that, if any water baby were ever found, it would be divided between Professors Huxley and Owen for examination (p. 44).[9] How well Kingsley kept in touch with scientific developments is indicated by his incidental reference to *Archæopteryx* (p. 47), discovered only shortly before his fairy tale went to press. Many a nursemaid must have read to her charges the account of how Professor Ptthmllnsprts (presumably "Put-them-all-in-spirits") "got up once at the British Association, and declared that apes had hippopotamus majors in their brains as men have" (p. 94) – but perhaps few of them recognised the reference to Huxley's 1860 assertion that the brain of the ape contained a *hippocampus minor.* And, as Kingsley continued, this was a shocking thing to say, "for, if it were so, what would become of the faith, hope, and charity of immortal millions?" Even more illuminating is the account of "the great and famous nation of the Doasyoulikes," living "at the foot of the Happy-go-lucky Mountains, where flapdoodle grows wild," who by a slow process of

[9] Page references are to the Watergate Classic ed. (1948).

retrogressive evolution eventually became apes. "Yes," said the fairy solemnly, "there are two sides to every question, and a downhill as well as an uphill road; and if I can turn beasts into men, I can, by the same laws of circumstance, and selection, and competition, turn men into beasts" (p. 140).

Huxley told Kingsley, "I am quite as ready to admit your doctrine that souls secrete bodies as I am the opposite one that bodies secrete souls – simply because I deny the possibility of obtaining any evidence as to the truth or falsehood of either hypothesis . . . If you tell me that an Ape differs from a Man because the latter has a soul and the ape has not, I can only say that it may be so . . . until you satisfy me as to the soundness of your method of investigation, I must adhere to what seems to my mind a simpler form of notation – *i.e.*, to suppose that all phenomena have the same substratum (if they have any), and that soul and body, or mental and physical phenomena, are merely diverse manifestations of that hypothetical substratum" (22 May 1863). As for retrogressive evolution, Huxley was in agreement that "So far from a gradual progress towards perfection forming any necessary part of the Darwinian creed, it appears to us that it is perfectly consistent with indefinite persistence in one state, or with a gradual retrogression."[10]

Nevertheless, this fallacious notion of inevitable progress towards perfection as a necessary corollary of natural selection greatly helped the acceptance of Darwinism. The validity of a scientific theory is nowise determined by its consonance or conflict with popular opinion, or by the readiness of experts to move in its direction, or by the personal characters of its proponents or opponents, or by its convenience as ideological camouflage for vested interests. All these, however, may markedly influence the speed with which a new theory gains ground, and this was conspicuously the case with Darwin's theory. Had there not been widespread readiness to accept some sort of evolutionary ideas; had not the geological work of Lyell and others prepared the minds of scientists for the idea that massive changes can be brought about by small causes granted only sufficient time; had not Darwin already established a reputation for meticulous work and earned the admiration of fellow-scientists for his conscientiousness and their affection for his modesty, and had not Huxley been so brilliant an advocate and so dauntless an agent-general; and had not the idea of "the survival of the fittest" so admirably provided a sort of pseudo-scientific sanction for the apologists

[10] "Criticisms on 'The Origin of Species,'" *Collected Essays*, II, 90-91.

of laissez-faire capitalism, there is no knowing how long the opposition to Darwin might not have maintained its strength. As things were, the worst of the abuse was spent within a decade, and after another decade what opposition remained was thoroughly respectful. Several genuine gaps in Darwin's evidence happened to be quite quickly filled – the discovery of *Archæopteryx* provided one of the hypothecated "missing links," study of *Amphioxus* cast light upon the borderland between vertebrate and invertebrate, embryological research went apace, and the massively accumulating fossil evidence (especially in America) tended to crush opposition by its mere weight. And, with geologising become the favourite hobby of half the country clergymen, criticism was disarmed where it might most have flourished.

As early as 1863 Kingsley was writing to Maurice, "Darwin is conquering everywhere and rushing in like a flood, by the mere force of truth and fact."[11] What was really happening was that the general idea of evolution was running in like a flood, Darwin having opened the floodgates, but the flood was not pure Darwin and the force was not merely that of truth and fact. In 1861 came Maine's *Ancient Law*, in 1863 Huxley's *Man's Place in Nature* and Lyell's *Antiquity of Man*, in 1865 McLennan's *Primitive Marriage* and Tylor's *Early History of Mankind*, in 1866 Marx's *Kapital* and Bagehot's *English Constitution*. Evolutionary assumptions and ways of thinking rapidly spread through anthropology, history, and theology, and in the process Darwin's own great personal contribution became obscured. "I cannot understand why you scientific people make such a fuss about Darwin," remarked Matthew Arnold to John Judd in 1871, "Why, it's all in Lucretius."[12] In the same year St. George Mivart published his *Genesis of Species* and claimed that evolution was not merely compatible with Roman Catholicism but actually approved by the Jesuit Father Suarez – a claim which at once sent Huxley to the Latin text and led him, as he said, to "come out in the new character of a defender of Catholic orthodoxy, and upset Mivart out of the mouth of his own prophet."[13] In his "Mr. Darwin's Critics" Huxley declared roundly, "If Suarez has rightly stated Catholic doctrine, then is evolution utter heresy. And such I believe it to be" (*Collected Essays*, II, 147). Huxley was quite right, but it is an interesting commentary on the trend of opinion that within twelve years of the

[11] *Charles Kingsley: His Letters and Memories of His Life*, ed. Mrs. Kingsley, abr. version (1883), p. 253.
[12] J. W. Judd, *The Coming of Evolution* (1910), p. 3.
[13] *Life and Letters of Thomas Henry Huxley*, ed. Leonard Huxley (1900), I, 392.

appearance of Darwin's book he had to be *proclaiming* its heresy. Nine years later he reminded the Royal Institution that "it is the customary fate of new truths to begin as heresies and to end as superstitions; and, as matters now stand, it is hardly rash to anticipate that, in another twenty years, the new generation . . . will be in danger of accepting the main doctrines of the 'Origin of Species,' with as little reflection, and it may be with as little justification, as so many of our contemporaries, twenty years ago, rejected them."[14]

Darwin's doctrines, unfortunately, were not simply accepted, but badly distorted. The idea that natural selection led inevitably to the "survival of the fittest" appealed greatly to the "Whites," who were so evidently surviving better than the "Blacks" in the world of the nineteenth century. So, just as in earlier days the apologists of slavery were able to quote selected Bible texts, now the spokesmen of "White" supremacy were able to pick passages from Darwin. To Darwin himself the slavery system was an abomination, and Huxley urged by lecture and in print that the results of miscegenation depended upon the social setting and that there was no reason to assume that primitive peoples were incapable of advancement,[15] but pseudo-Darwinian justification of racial discrimination continued to make headway. Similarly, despite Huxley's repeated insistence that intellectual ability was not the prerogative of any one social class, that "he did not believe that if 100 men were picked out of the highest aristocracy in the land and 100 out of the lowest class there would be any difference of capacity among them,"[16] that "the 'points' of a good or of a bad citizen are really far harder to discern than those of a puppy or a short-horn calf,"[17] the movement for eugenic reform developed a markedly anti-artisan bias, somewhat corrected only in the last couple of decades.

Even more crude was the distortion of Darwin to justify ruthless competitive capitalism. Men might starve, and women and children slave away for a mere pittance in the mines and mills, but all would be for the best in the end – the fittest would survive and the race thereby improve. Perhaps it is not the whole truth to call this the distortion of Darwin, for Herbert Spencer had arrived at evolutionary ideas quite independently and the phrase "survival of the fittest" was his own. As early as 1852 he had delivered to the British Association a paper "A

[14] "The Coming of Age of 'The Origin of Species'," *Collected Essays*, II, 229.
[15] *Birmingham Daily Post* (7-12 Oct. 1867).
[16] *Daily Chronicle* (8 June 1887).
[17] "Evolution and Ethics. Prolegomena," *Collected Essays*, IX, 23.

Theory of Population, deduced from the General Law of Animal Fertility," and in the same year he developed in the *Leader* a view of evolution much on Lamarckian lines. Unfortunately, once the opposition to Darwin had been thoroughly defeated and discredited, all sorts of bastard speculation seemed legitimised; and, as a friend of Darwin and Huxley, Spencer was able to appear to the undiscriminating as a spokesman of the new knowledge. When Huxley told the Birmingham and Midland Institute, as his "Administrative Nihilism" (1871) put it, that the extreme laissez-faire view favoured "neither a monarchy, an aristocracy, nor a democracy, but an *astynomocracy*, or police government" (*Collected Essays*, I, 259), Spencer developed an attack on his friend. Seventeen years later the two were still at loggerheads on this issue, and Huxley's "Struggle for Existence in Human Society" (*Collected Essays*, IX, 195-236) led, as he had realised that it would, to a quarrel with Spencer. From the point of view of the moralist, Huxley maintained, the animal world is at about the level of a gladiators' show, and the ethical man must not fight out the struggle for existence like any other animal but devote his best energies to setting limits to the struggle. As late as 1893, in his Romanes Lecture on "Evolution and Ethics," he was still trying to combat this sort of misapplication of evolutionary theory, but the Spencers and the Nassau Seniors won. As Huxley remarked, in language worthy of Engels, "men have become absorbed in the mere accumulation of wealth; and as this is a matter in which the plainest and strongest form of self-interest is intensely concerned, science (in the shape of Political Economy) has readily demonstrated that self-interest may be safely left to find the best way of attaining its ends."[18]

The impact of the *Origin of Species* was immense, but it was not only scientific and theological orthodoxy which it shattered. Everything was put into flux, and some of the directions of flow were in an ethical and political sense reactionary. Now, a century later, we have the task of disentangling the true from the false, the applications from the misapplications, the good effects from the bad effects, and a mighty task it still is.

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[18] "Administrative Nihilism," *Collected Essays*, I, 268.

Edward Lurie

LOUIS AGASSIZ AND THE IDEA OF EVOLUTION

THE IDEA OF ORGANIC EVOLUTION met with no more articulate or intellectually competent opponent than Louis Agassiz.* Long before the *Origin of Species* appeared, this advocate of the glacial theory and author of the renowned *Poissons fossiles* had expressed his opposition to all ideas of "development" or change in the natural world. It was consistent with his education and past experience that he found Charles Darwin's book to represent "a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency."[1] Agassiz never deviated from the strength of this conviction. Despite significant evidences for evolution advanced after 1859, and in the light of his own strong powers of observing differences in animate creation, he could still believe in 1873 that Darwin's theories were "merely conjectural" and that the English naturalist had "not even made the best conjecture possible in the present state of our knowledge."[2]

Asa Gray evaluated this intellectual condition when he wrote of Agassiz's career as "singularly entire and homogenous – if not uninfluenced yet quite unchanged by the transitions which marked the period."[3] Some contemporaries who knew the power of Agassiz's descriptive and analytical mind never could understand his failure to see the truth of the evolution idea. Jeffries Wyman, a Harvard colleague of

* I am indebted to Professor Ernst Mayr, who kindly supplied me with the typescript of his article, "Agassiz, Darwin, and Evolution," *Harvard Library Bulletin*, XIII (1959), 165-194. My understanding of Agassiz's relationship to the evolution idea has been greatly enhanced by Professor Mayr's perceptive analysis.

1 "Professor Agassiz on the Origin of Species," *American Journal of Science*, 2nd ser., XXX (1860), 154.

2 "Evolution and Permanence of Type," *Atlantic Monthly*, XXXIII (1874), 101.

3 "Louis Agassiz," *Andover Review*, Jan. 1886, p. 38.

Agassiz and a man who furnished Darwin with important evidence, expressed his perplexity when he wrote, "Agassiz . . . was just the man who ought to have taken up the evolution theory and worked it into a good shape, which his knowledge of embryology and paleontology would have enabled him to do."[4] Joseph Le Conte, a devoted student of Agassiz's and himself a disciple of Darwin, even went so far as to claim for his master the discovery of principles that were, he believed, the only true basis for a theory of evolution, namely, that the life history of the embryo repeated the entire succession and development of its class in past ages.[5] As Wyman understood it, Agassiz's identification of intermediate forms, "prophetic" and "synthetic" types that suggested in their appearance the course of future development, was the kind of insight that might well have led him to an appreciation of Darwin's genius. "It has even been said that I have myself furnished the strongest evidence of the transmutation theory" ("Evolution and Permanence of Type," pp. 100-101), Agassiz ironically acknowledged in the last year of his life.

Agassiz was so little impressed by Darwin's contribution of 1859 that he would not even grant it the status of an original theory. This, he insisted, was speculative, old-fashioned, romantic biology that proceeded from impressing a priori convictions upon the facts of nature, rather than allowing the facts to speak for themselves in the true spirit of science. All of this had been debated and rejected before in the outworn theories of Lamarck, Oken, and the anonymous author of *Vestiges of the Natural History of Creation*. It was hardly worth the bother of another effort to demolish such fanciful speculations. Scientists should get on to the real business of their craft, precise and exact description and comparison, instead of wasting valuable time and effort chasing such phantoms of imagination and fanciful design. It was distressing for a man in the prime of his scientific life to find colleagues and students so impressed by an idea he had already analyzed and found hardly worthy of attention. In 1873 it seemed to Agassiz that the scientific world was chasing will-o'-the-wisps: "The more I look at the great complex of the animal world, the more sure do I feel that we have not yet reached its hidden meaning, and the more do I regret that the young and ardent spirits of our day give themselves to speculation rather than to close and accurate investigation" ("Evolution and Permanence of Type," p. 101). An even more graphic sign of the changing world occurred in 1869 when Agassiz had to introduce his own son, whom he had personally trained

[4] Quoted in Dirk J. Struik, *Yankee Science in the Making* (Boston, 1948), p. 312.
[5] *Evolution and its Relation to Religious Thought* (1888), pp. 32-49.

as a naturalist, to Darwin with the words, "You will find Alex more ready to accept your views than I shall ever be."[6]

Agassiz dissented from the developing trend of scientific opinion in his own day with such single-minded devotion to the principles of his education and to the force of a deeply rooted natural theology that in 1863 Asa Gray could comment that the idea of evolution had "only one thoroughgoing and thoroughly consistent opponent . . . Mr. Agassiz."[7] Consistency of viewpoint alone did not make Agassiz's reaction to this idea unique. Agassiz represented, in microcosm, the full sweep of scientific conviction and philosophical certitude that affirmed the timeless permanence of the universe, a stability that shone through every observed fact of nature, such immutability being derived from the immaterial intelligent plan of creation designed by the Deity.

Agassiz was fully conscious of the responsibility derived from his assumed role as the contemporary defender and advocate of an intellectual tradition that stretched back to Plato and Aristotle and found more recent expression in the thought of Schelling and Cuvier. So powerful was this sense of obligation that he would even expand the generally accepted province of the Deity in formal theology. If it were necessary to defend the permanence of species through denying any possibility of relationship and descent, he would insist that animals and plants had been separately created where they were found at present, and that such creation had occurred time and time again. If this seemed to deny Biblical interpretation, then such contradiction was necessary and proper, for the naturalist who believed in the supreme power of the Deity was defending a higher principle than the trustworthiness of mere written accounts or human interpretations. It was thus possible for Agassiz to assert:

> The most advanced Darwinians seem reluctant to acknowledge the intervention of an intellectual power in the diversity which obtains in nature, under the plea that such an admission implies distinct creative acts for every species. What of it, if it were true? Have those who object to repeated acts of creation ever considered that no progress can be made without repeated acts of thinking? And what are thoughts but specific acts of the mind? Why should it then be unscientific to infer that the facts of nature are the result of a similar process, since there is no evidence of any other cause? ("Evolution and Permanence of Type," p. 101)

With these convictions, Agassiz's thought might appear as merely

[6] Quoted in G.R. Agassiz, ed., *Letters and Recollections of Alexander Agassiz* (1913), p. 97.

[7] *Darwiniana: Essays and Reviews Pertaining to Darwin* (1876), p. 199, reprinting an article of May 1863 in the *American Journal of Science*.

the dogmatic expression of a traditional idealism confronted with a method and viewpoint in both natural history and philosophy that denied all the verities he held as controlling basic assumptions. Yet in the years before 1859 Agassiz had not only been educated to appreciate the importance of the dictum "a physical fact is as sacred as a moral principle," but had also contributed in notable fashion to the advance of general and special knowledge. Between 1833 and 1843 he had described in minute and careful detail over 1700 species of fossil fishes, identifying their stratigraphic location and taxonomic relationship.[8] Between 1837 and 1847 he had advanced the theory that large parts of Europe and North America had once been covered by a gigantic sheet of ice, of which glaciers were the contemporary remnants and moraines and scratched and polished rocks the modern signposts.[9] He had, moreover, undertaken highly competent studies in the embryology and zoological character of marine animals, correlating such data with material drawn from the field of invertebrate paleontology. By 1849 he had perceived, as had others before him, that there was undeniably a process in nature whereby "earlier animals correspond not only to lower types of their respective classes, but [also] that their chief peculiarities have reference to the modifications which are successively introduced during the embryonic life of their corresponding representatives in the present creation."[10] Knowledge of embryology had shown him, moreover, that change was forever present in the development of the egg to its adult form. Understanding of paleontology had revealed much new information on the classification and description of fossils from anatomical principles derived from contemporary forms. From all these accomplishments and more, it could hardly seem that Agassiz was not an observer of the first order, entitled to the Cuvier Prize of the French Academy of Sciences and the Wollaston Medal of the Geological Society of London. This was a man who read and re-read Aristotle every two or three years because he found his ideas "charming," and who at the same time was able to characterize fossil fishes with an exactitude and precision that was never equaled in the nineteenth century.

[8] *Recherches sur les poissons fossiles . . .*, 5 vols. (1833-43).

[9] See, e.g., *Etudes sur les glaciers* (1840).

[10] *Twelve Lectures on Comparative Embryology* (1849), p. 27. The earliest statement of this viewpoint occurs in a letter to Alexander von Humboldt, July 1832, in Elizabeth Cary Agassiz, ed., *Louis Agassiz . . .*, 2 vols. (1885), I, 203-204. Agassiz expressed the same general idea in *Poissons fossiles*, I, 102, and in other writings in paleoichthyology. His full relationship to this concept is worthy of a separate analysis. See the valuable treatment of Darwin's relationship to Agassiz in this context by Jane Oppenheimer, "An Embryological Enigma in the Origin of Species," in Bentley Glass *et al.*, eds., *Forerunners of Darwin: 1745-1859* (Baltimore, 1959), pp. 292-322.

The root of Agassiz's intellectual and personal dilemma in his relationship to the theory of evolution was that his overpowering idealism would never let him admit that nature could be the product of chaotic, unplanned, "physical forces." Since man could understand nature by virtue of his intelligence, there must be an immaterial thread connecting this mental act with the operations of a higher Mind responsible for all animate creation. At the same time, there were the observed facts of nature, facts which suggested derivation and relationship to men like Darwin, Hooker, and Huxley. How could these facts be accounted for? "The difference between myself and Darwin is entirely owing to different ways of looking at the same thing," Agassiz candidly confessed to his students.[11] It was thus imperative for the preservation of Agassiz's entire scheme of the universe that the phenomena of nature be referred to and explained by the operations of thought and intelligence.

The result of this conviction was two-fold: Agassiz was forced at times to deny, categorically, the objective existence of evidence that suggested transmutation of species. Such denial often had an almost pathetic cast, especially as the weight of observed derivation and relationship gave more and more substance to opposite convictions. Secondly, Agassiz was forced at the same time to manipulate observed phenomena into the grand design of a planned and forever unchanging universe. Thus "prophetic types" became not transitional forms but evidence supplied by the Creator regarding his intentions for the future. Glacial action and the ice age were interpreted not as the secondary agents responsible for the contemporary distribution of certain flora and fauna, or signs suggestive of the manner in which the past history of the earth could be explained by reference to the present, but rather as further spectacular demonstrations of the power of the Deity in causing great catasphophes, events which had made impossible genetic relationships between forms existing prior to and after the ice age. Empiricism was employed in yet another effort to contradict Darwin. Agassiz's acknowledged competence in paleontology and zoology enabled him to seize upon every inconsistency, unexplained fact, or absence of proof with a perception derived both from his dedication to precise observation and the spiritual compulsion to show Darwin to be wrong.

Agassiz was thus a formidable opponent for Darwin, and the

[11] Anon. [class notes on evolution lectures by Agassiz], 10 Mar. 1871, Agassiz Papers, Museum of Comparative Zoology, Harvard University.

English naturalist realized the power of such opposition when he predicted to the American geologist James Dwight Dana in 1856, "Agassiz, if he ever happens to read my work, will throw a boulder at me."[12] Darwin understood that Agassiz represented both the forces of philosophical idealism and the conviction of many established naturalists. In so representing these currents of thought, Agassiz's reaction to the evolution idea symbolized the wide-ranging individual reactions of colleagues in Germany, France, and England. What was worse, from Darwin's viewpoint, was the fact that Agassiz was a gifted popularizer of natural history, with a long record of charming and delightful performances to audiences of many intellectual levels. He seemed a powerful figure indeed.

In November 1859 Charles Darwin sent Agassiz a presentation copy of the *Origin of Species*. "My Dear Sir," he wrote in an accompanying note, "I have ventured to send you a copy of my book. . . . As the conclusions at which I have arrived on several points differ so widely from yours, I have thought (should you at any time read my volume) that you might think I had sent it out of a spirit of defiance or bravado; but I assure you that I act under a wholly different frame of mind. I hope you will at least give me credit, however erroneous you may think my conclusion, for having carefully endeavored to arrive at the truth."[13] Agassiz read the book with interest, and growing annoyance. "This is truly monstrous!" he wrote in the margin next to a passage he found especially offensive. Darwin had departed from the methods of scientific inquiry so well exemplified in his earlier studies; he had contributed nothing new to the understanding of nature. He had, in Agassiz's opinion, simply revived in another form the exploded theories of earlier speculation about "development" from common ancestry. "What is the great difference between supposing that God makes variable species or that he makes laws by which species vary?" Agassiz asked himself in another marginal note. His final evaluation was at the same time a prediction of future assessments. He refused to give Darwin any credit for utility or originality of conception. "What does [all] this prove except an ideal unity holding all parts of one plan together?"[14]

The sources of Agassiz's convictions that led him to dismiss Dar-

[12] 29 Sept. 1856, James Dwight Dana Papers, Rare Book Room, Yale University Library.
[13] [11] Nov. 1859, Agassiz Papers, Houghton Library, Harvard University.
[14] Marginal notes on pp. 167, 184, and 194 of Agassiz's copy of the *Origin of Species*, on deposit in the rare book cases, Library of the Museum of Comparative Zoology, Harvard University.

win so easily are significant, because they provide revealing indexes to the kind of philosophic and scientific opposition Darwin encountered both in America and Europe. Agassiz's formative years had been shaped by experiences at the University of Munich. When Agassiz came to Munich from Switzerland in 1827, it had an outstanding faculty in science and philosophy. Three scholars in particular shaped the thought of the young student who even then aspired to be "the first naturalist of his time." Ignaz Döllinger, a highly gifted anatomist and embryologist and the teacher of Karl Ernst von Baer, showed Agassiz how to examine nature minutely, in its parts, as the primary condition for understanding the world as a whole. By the use of the microscope and the dissecting knife Agassiz acquired an abiding respect and appreciation for the inner reality of nature. To observe the development of the egg, to study the life history of vertebrates, to examine the genesis of tissues gave him the feeling that he stood on the threshold of ultimate knowledge. At the same time, he learned from those stellar representatives of *Naturphilosophie*, Lorenz Oken and Friedrich Schelling, that nature "herself reveals the plan of her thoughtful operations."[15]

Agassiz was thus introduced to two viewpoints that shaped much of his thinking about natural history. Every physical fact possessed a particular significance and also revealed a higher purpose. There was, first, the importance of studying nature exactly to determine the quality and character of the object under examination, and if nature meant anything at all, it had a cosmic dimension. It mattered little that a teacher like Oken suggested this purpose to be so uniform throughout the animal kingdom that it demonstrated a series of physical relationships common to lower and higher animals. Agassiz had learned from Döllinger that change and development occurred constantly in nature; but such transformations were individual, an unfolding of life forms from the egg to the adult. Such change, moreover, always resulted in a mature form, and this form was fixed, final, and unrelated to any that preceded it. Methods for examining nature empirically and a conviction that thoughtful planning had inspired such specific evidence, these were the most important fruits of Munich for Agassiz.

Secondly, Agassiz inherited from Baron Georges Cuvier, whom he met in Paris in 1832, an attitude toward nature in which flora and fauna

[15] Johannes Müller, quoted in Owsei Temkin, "German Concepts of Ontogeny and History Around 1800," *Bulletin of the History of Medicine*, XXIV (1950), 237. This analysis (pp. 227-246) is an admirable treatment of *Naturphilosophie* and related concepts.

were regarded as products of Divine thought.[16] In Agassiz's words, "Nature is the work of thought, the production of intelligence, carried out according to plan. . . . and in our study of natural objects we are approaching the thoughts of the Creator . . ."[17] Agassiz added substance and content to this framework by subsuming the objective evidence of natural science under an idealistic world view. Later this process took form through Agassiz's systematic and rational categories of nature, conceptions that rested on a logic derived from Plato and Aristotle as well as from Cuvier and his Munich education.

Such categories were the basis of Agassiz's spiritual taxonomy since "Classification, rightly understood means simply the creative plan of God expressed in organic forms."[18] Agassiz, like Cuvier, identified four grand divisions of the animal kingdom, categories that contained entirely independent and unrelated faunal types, each identifiable by a distinct structural form and plan. This scheme of creation was as true for ancient times as for the present, and all four divisions had representatives in the earliest geological formations. These divisions or branches (radiates, mollusks, articulates, and vertebrates) were each subdivided into smaller units of identity: classes, orders, families, genera, and species. There were no genetic relationships between each division: the structural plan upon which each type was based was as immutable as the Divine intelligence that had inspired them. Moreover, there were no derivative affinities between modern and ancient representatives of the same division, because of the intercession of catastrophes, by which the Creator had wiped out all life in one epoch and started anew, again and again, the grand creative scheme. Ancient fishes and mammals of the Tertiary Period could not conceivably be related to fishes or men of the modern age, because of the catastrophe of the great glacial epoch. Life started anew, successively, and it was a hardy fish, therefore, who could survive Agassiz's ice.

Agassiz's definition of species, the individual unit of existence in nature, was of fundamental importance. Species, like other grand divisions of the animal kingdom, were ideal representatives of the creative plan. Individuals were the material duplications of such Divine ideas. Individuals might change or vary; but variation was not to be interpreted as variation in the Divine form. While one species bore no genetic rela-

[16] Agassiz's intellectual indebtedness to Cuvier is apparent in almost everything he wrote. See, for example, *An Essay on Classification* (1859), pp. 75-77, 214-220, 306-310.
[17] "Methods of Study in Natural History," *Atlantic Monthly*, IX (1862), 13.
[18] *Methods of Study in Natural History* (1863), p. 42.

tionship to other species, all shared a common origin in the mind of the Creator: "categories of thought, embodied in individual living forms."[19] The Deity, moreover, had quite plainly intended only one of His powerful demonstrations – the creation of man himself – to be the capstone of His creative effort. The only progress man could make was moral and intellectual, and Agassiz was pleased to assist in this effort by demonstrating the affinity of the human soul with the higher power that operated through all nature.

The scope of Agassiz's scientific efforts prior to 1859 revealed an increasing conviction that such an interpretation of nature was the only reasonable one in the light of the evidence. Geology, paleontology, embryology, ichthyology, and geographical distribution were all subjects of intense interest to Agassiz as they were to Darwin. In each field of inquiry he found evidence to substantiate his natural theology.

Examples from two fields of inquiry reveal the pattern of Agassiz's thought. In the decade from 1847 to 1857 Agassiz undertook the task of applying the assumptions of special creationism to the subject of human origins. Taking issue with the traditional belief of eighteenth-century biology that man was one species and had originated from a single pair in a common center of creation, Agassiz affirmed the opposite to be true, that "the differences between the races were primitive" and that man had not been created "from a common center or a single pair."[20] To substantiate this novel conception Agassiz identified particular zoological provinces, areas that were limited and distinct, containing separate, mutually exclusive animal and human types. Revealingly, he argued that a belief in the separate origin and independent creation of both human and animal species was necessary to demonstrate the supreme power of the Deity and to disprove any "developmental" or evolutionary view of natural history. Concepts of development were patently false, speculative notions. It followed, therefore, that the concept of unity of origin for any species, man or animal, was "contrary to all the results of modern science."[21]

Then, in 1857, Agassiz published the first two volumes of a pro-

[19] *Contributions to the Natural History of the United States*, 4 vols. (1857-62), III (1860), 88.

[20] American Association for the Advancement of Science, *Proceedings*, III (1850), 107.

[21] "Sketch of the Natural Provinces of the Animal World and their Relation to the Different Types of Man," in J. C. Nott and G. R. Gliddon, *Types of Mankind* (1854), p. lxxviii. For a full discussion of this question see Edward Lurie, "Louis Agassiz and the Races of Man," *Isis*, XLV (1954), 227-242. A perceptive commentary on this scheme

jected ten-volume study, *Contributions to the Natural History of the United States*. This effort, he promised, would represent the intellectual labor of a decade and would supply new data and meaningful interpretations of complex questions regarding zoological classification, the geographical distribution of animals, and the problem of relationship in natural history.[22] Such promises were not fulfilled. The first part of the first volume contained the most important part of the entire study, an "Essay on Classification," offered as a much more utilitarian taxonomic system than those currently in use. The "Essay," however, was more of a treatise in natural theology than in natural history. While revealing Agassiz's comprehensive knowledge of the history of biology, and providing a precise and detailed summary of past investigations in various fields, nearly every page reflected a reiterative testimony of faith in the teachings of Cuvier and logic of classical biology. Every observed fact of ancient and contemporary natural history demonstrated thoughtful planning and an intelligence reflecting "premeditation, power, wisdom, greatness, prescience, omniscience, providence. . . . all these facts . . . proclaim aloud the One God, whom man may know, adore, and love; and Natural History must, in good time, become the analysis of the thoughts of the Creator of the Universe, as manifested in the animal and vegetable kingdoms, as well as in the inorganic world."[23] Here, then, was a truly remarkable demonstration of the power of thought over matter. Wherever Agassiz's searching intellect turned in the scheme of nature, empirical study yielded confirmation of a priori truths.

It is clear that Agassiz's method had presupposed the results he had achieved. He had not superimposed a system on nature in the manner of *Naturphilosophie*. Instead he had let the facts speak for themselves through his perception in a way that symbolized the correspondence of human intellect with the thought of the Creator. He could thus write with pride that "this growing coincidence between our systems and that of nature shows . . . the identity of the operations of the human and

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of creation was offered by the English geologist Sir Charles Lyell who later wrote, "I must confess Agassiz's work drove me far over into Darwin's camp . . . for when he attributed the original of every race of man to an independent starting point, or act of creation, and . . . created whole 'nations' at a time, every individual out of 'earth, air, and water' the miracles really became to me so much in the way of S. Antonio of Padua that I could not help thinking Lamarck must be right, for the rejection of his system led to such license" (Lyell to George Ticknor, 9 Jan. 1860, in Katherine M. Lyell, ed., *Life, Letters and Journals of Sir Charles Lyell, Bart.*, 2 vols. [1881], II, 331).

[22] "Prospectus" and "Private Circular" in Agassiz to Lewis R. Gibbes, 28 May 1855, Lewis R. Gibbes Papers, Manuscripts Division, Library of Congress; *American Journal of Science*, 2nd ser., XX (1855), 149-151.

[23] *An Essay on Classification*, p. 205. The 1859 reprint of the "Essay" is cited throughout for the sake of simplicity.

Divine intellect; especially when it is remembered to what an extraordinary degree many *à priori* conceptions . . . have in the end been proved to agree with reality, in spite of every objection at first offered . . . by empiric observers" (*Essay on Classification*, p. 31). Agassiz was compelled, therefore, by his training under Döllinger and Cuvier, to affirm the grandeur of "the domain of facts," which he felt had been all too seldom honored by "physio-philosophers" like Oken and the author of *Vestiges of Creation*. Discarding what seemed the all too suggestive mechanistic and materialistic implications of "development," which he perceived in certain aspects of his German education, Agassiz embraced a view of nature equally subjective: "There will be no *scientific* evidence of God's working in nature until naturalists have shown that the whole Creation is the *expression of a thought*, and not the *product of physical agents*."[24]

Agassiz was thus able to employ the twin-edged sword of empirical evaluation and metaphysical rebuttal in his self-appointed mission to exemplify the intellectual operations of mind in nature. It was entirely possible for him to examine a great variety of data, much of it of obvious evolutionary significance, and find all such evidence demonstrations of the creative plan. Whether he considered the material of zoology, embryology, or paleontology, the results were all the same. For example, in regard to the crucial problem of geographical distribution and separation, a concept that was providing Darwin and others with important insights regarding variation and community of descent, Agassiz found: "The distribution of some types over the most extensive range of the surface of the globe, while others are limited to particular geographical areas . . . exhibits thought, a close control over the distribution of the earth's surface among its inhabitants" (*Essay on Classification*, pp. 200-201). The same thought was demonstrable in "the parallelism between the order of succession of animals in geological times and the changes their living representatives undergo during their embryological growth . . ." (*Essay on Classification*, p. 203). Creation according to a thoughtful plan could be discovered alike in the ancient history of the earth or the contemporary relations of animals and plants. It was this determination to ascribe such grandiose powers to the Deity that made Agassiz reject any interpretation that seemed to imply, even by a stretching of the imagination, that "physical agents" or "physical causes" could be responsible for the appearance of nature.

In January 1859 Agassiz gave these views even wider circulation

[24] *Lake Superior . . .* (1850), p. 145.

when he published the "Essay on Classification" as a separate volume. It was published in London, at the urging of "friends in whose opinion I have great confidence." Agassiz was a highly respected figure in English scientific circles, notably for his research on the fossil fishes of the Old Red Sandstone, and the ideas of the *Essay* were held in high regard by such dignitaries of natural history as the geologist Adam Sedgwick and the anatomist Richard Owen.[25] In the summer of 1859 Agassiz visited his English colleagues and was accorded a great reception. Even Huxley was impressed by the "heartiness and candour" of the American visitor, although both men appeared to have agreed to disagree on the major questions of the day in natural history.[26]

Agassiz had satisfied himself, by 1859, that he had said all there was to say with regard to the central questions of natural history. But eminent American colleagues, men of the caliber of the geologist James Dwight Dana and the botanist Asa Gray, thought otherwise. For during the decade 1847 to 1857 these men became more doubtful of the validity and utility of the concept of special creationism. Dana, for example, reviewed the first volumes of the *Contributions* in the *American Journal of Science*. He naturally centered his attention on the "Essay on Classification." Admitting that there was much in these pages that showed careful labor, he doubted that such a system of classification as Agassiz advanced was really useful in actual research. Limitation of zoological forms to distinct zones of creation was far too strict a viewpoint, and the idea of the origin of species from many different centers of creation was still very much of an open question.[27]

The growing professional dissatisfaction with Agassiz's view of nature had emerged in public debate in January 1859. The occasion was a meeting of the American Academy of Arts and Sciences in Boston, where Asa Gray presented a paper designed to "knock out the underpinning of Agassiz' theories about species and their origin."[28] A comparative study of the botany of areas of North America with that of Japan led Gray to suggest that such forms had originated in a single, local center and then spread to different parts of the world "through changes in climate

[25] Adam Sedgwick to Agassiz, 10 Apr. 1845, 13 June 1859; Richard Owen to Agassiz, 9 Dec. 1857; Agassiz Papers, Houghton Library.
[26] Huxley to Agassiz, 11 July 1859, Agassiz Papers, Houghton Library.
[27] "Agassiz's Contributions to the Natural History of the United States," *American Journal of Science*, 2nd ser., XXV (1858), 202-216, 321-341.
[28] Gray to John Torrey, 7 Jan. 1859, Asa Gray Papers, Gray Herbarium, Harvard University. These are typed copies of originals on file at other depositories.

or physical conditions operating during a long period of time." As for Agassiz's belief in the multiple origin and separate, successive creation of species, Gray dispensed with this by affirming that "the facts of the case . . . appear to demand from science something more than a direct reference of the phenomena as they are to the Divine will."[29]

Agassiz's response to such fundamental criticism of views he had long held sacred was simply to deny the power of physical agents to affect change in nature, asserting that the distribution of animals and plants was accomplished according to a plan "hardly intelligible as yet to us."[30] It was inevitable, then, that the appearance of Darwin's volume would find inquiring American naturalists like Gray anxious that the work receive a full and fair hearing from the scientific community and that Agassiz would react to such discussions as he had in the past.

From February through April 1860 Agassiz was engaged in a series of discussions of the Darwinian hypothesis with associates in Boston. In debates with William Barton Rogers at the Boston Society of Natural History, and Asa Gray at the American Academy, Agassiz was placed in the increasingly uncomfortable position of arguing for a series of assumptions that could not be logically defended. Deeply held beliefs and controlling assumptions made it impossible for him to recognize the validity of any theory of nature but his own. For instance, in a discussion with Rogers, "Professor Agassiz . . . stated that he knew no such thing as a variety in the animal kingdom, except such as are stages of growth, within the limits of species . . . in 6,000 fishes he had not seen a variety except in coloration. . . . So that he would start with the propositions that animals do not vary, and that species remain within the limits of their type."[31]

Agassiz's personal reputation suffered as a result of these discussions. He failed to demolish Darwin's arguments or to dissuade his colleagues from a determination to investigate the facts of nature objectively. This determination was made plain in March 1860 when Gray published a review of the *Origin* in the *American Journal of Science*

[29] American Academy of Arts and Sciences, *Proceedings . . . 1857-1860*, IV, 132 (11 Jan. 1859).

[30] American Academy of Arts and Sciences, *Proceedings . . . 1857-1860*, IV, 178 (22 Feb. 1859). For the entire discussion of this and related matters see pp. 131-135 (11 Jan. 1859); 171-179 (22 Feb. 1859); 195-196 (22 Mar. 1859).

[31] Boston Society of Natural History, *Proceedings . . . 1859-1861*, VII, 271 (4 Apr. 1860). For the entire discussion see 231-235 (15 Feb. 1860); 241-245 (7 Mar. 1860); 246-249, 250-252 (21 Mar. 1860); 271-275 (4 Apr. 1860). For another, similar discussion, see American Academy of Arts and Sciences, *Proceedings . . . 1857-1860*, IV, 410 (6, 27 Mar. 1860); 411-415 (10 Apr. 1860); 424 (24 Apr. 1860); 424-426, 428 (1 May 1860).

which contrasted the positions of Agassiz and Darwin.[32] It must have been distressing for Agassiz to see his ideas set forth side by side with Darwin's – as if it were a question of deciding which view of nature to accept – when all his reasoning convinced him that permanence in creation was an objective reality that was merely reported by the perceptive naturalist! Yet Agassiz did not publish a public refutation of Darwin. He had other things on his mind: "I have never been more troubled by the pleasantest kind of pressure than I am now. . . . unpacking and arranging about one hundred thousand specimens acquired for the Museum within the last six months . . ."[33] Apparently, building up the collections of the Harvard museum was more vital than generalizing on their natural history. It seemed that he was either reluctant to publish his opinions or that the popularization of science and the establishment of a museum was more important than intellectual interchange with scientific colleagues.

In July 1860 Agassiz finally responded to the threat of Darwin and the arguments posed in the Boston debates with an article reviewing the *Origin* in the same journal that had carried Gray's appraisal. He observed that "the arguments presented by Darwin have not made the slightest impression on my mind, nor modified in any way the views I have already propounded" ("Professor Agassiz on the Origin of Species," p. 143). Darwinism would go the way of the false doctrines of Lamarck, Oken, and the idea of spontaneous generation. Once more "physical agents" were identified with chaos. The only reasonable approach was to "look to the original power that imparted life to the first being for the origin of all other beings" ("Professor Agassiz on the Origin of Species," p. 149). There was an obvious and simple choice confronting naturalists, one not cast in the terms Gray had delineated, but alternatives that ranged the Supreme Intelligence and empirical evidence on the one side, and "speculation," "deduction," and a world in flux and without purpose on the other. It was as if the salient points of the *Essay on Classification* had been digested, without the impressive bibliography or sketches of prior systems of classification, and Darwin substituted for all of the eighteenth- and early nineteenth-century villains of the former work.[34]

[32] "Review of Darwin's Theory on the Origin of Species by Means of Natural Selection," *American Journal of Science*, 2nd ser., XXIX (1860), 153-184.

[33] Agassiz to Thomas G. Cary, Jr., 26 Mar. 1860, Agassiz Papers, Houghton Library.

[34] Darwin was actually disappointed that he did not meet stronger opposition. He wrote Gray, "I am surprised that Agassiz did not succeed in writing something better . . . it is

In October 1860 another discussion between Agassiz and Gray took place at the American Academy, with the significance of variability once more the main topic. Agassiz's remarks showed how firmly the idea of species as absolute categories of thought prevented him from recognizing that individual variation, an observed phenomenon he knew from careful study, could ever result in the transmutation of species.[35] If "species" were merely arbitrary designations supplied by the naturalist, as Darwin maintained, then thought and intelligence were forever banished from the world. Agassiz had asked Darwinians the perfectly logical question: "If species do not exist at all, as the supporters of the transmutation theory maintain, how can they vary? And if individuals alone exist, how can the differences which may be observed among them prove the variability of species?" ("Professor Agassiz on the Origin of Species," p. 143). It seemed that both Agassiz and his opponents had arrived at an intellectual impasse.

This division of opinion became sharper and more public in October 1860 when Agassiz's review of Darwin was reprinted in the third volume of the *Contributions* (pp. 88-99 and 112-113). In one year Agassiz's opinion had not changed in the slightest from his initial reaction of 1859 or from the general lines of interpretation summarized in the *Essay on Classification*.[36] The power of Agassiz's education and the force of his subjective approach to nature made a new theoretical orientation impossible. The result was that after 1861 Agassiz effectively isolated himself from the main lines of research and generalization in an intellectual domain where he had been an acknowledged master for over thirty years. His reputation declined from 1861 onward, and he made only rare efforts to defend his position within the framework of intellectual debate. He preferred instead to carry on his refutation in the forum of public opinion, underscoring more sharply his isolation from scholarly interchange. His increasing absorption in public activity and the administrative aspects of science made it impossible to approach

no wonder that a man who calls identical forms when found in two countries, distinct species, cannot find variation in nature . . ." (11 Aug. [1860], Historic Letter File, Gray Herbarium). Sir Charles Lyell, always a perceptive observer, commented: "Agassiz helped Darwin . . . by . . . not hesitating to call in the creative power to make new species out of nothing whenever the slightest difficulty occurs of making out how a variety got to some distant part of the globe" (Lyell to George Ticknor, 29 Nov. 1860, in *Life . . . of Lyell*, II, 342).

[35] American Academy of Arts and Sciences, *Proceedings . . . 1860-1862*, V, 72 (9 Oct. 1860).

[36] Despite the theological character of Agassiz's opposition, he had asked Darwin and his supporters many pertinent questions. Darwin had benefited from Agassiz's contributions to geology and zoology before 1859, and, if collaboration had been possible, it could have been fruitful indeed.

Darwin's work with arguments any different from those advanced in the period 1859-61. Agassiz thus assumed a responsibility for showing the public the fallacies of Darwinism through evidence and interpretations already found insufficient by his scientific peers.

Agassiz's popular mission to rescue Americans from the Darwinian heresy was undertaken most vigorously in the period from 1861 to 1868. In these years he delivered eight series of public lectures, wrote twenty-five popular articles, and published three books, all designed to establish the public acceptance of a philosophy of nature that was the antithesis of Darwin's view.[37] The total result, from the viewpoint of science, was worthless. In fact, Agassiz's determination to combat Darwin led him to undertake investigations that were demonstrably inferior to scientific labors of former years. In 1865 he journeyed to Brazil and claimed that large portions of the Amazon River valley and coastal areas near Rio de Janeiro exhibited signs of glacial action similar to the history of the ice age in Europe and North America. It followed that the flora and fauna of this region had been created anew after the glacial ice had receded. Consequently there could be no possible genetic relationship between animals and plants that had lived prior to and after the ice age.[38] The extension of the glacial theory was a preconceived effort to find further means to disprove Darwin. It was based on very inconclusive evidence and only limited field study, efforts hardly comparable to Agassiz's geological investigations of former years. Darwin and his defenders quite naturally interpreted this unconvincing research as proof that Agassiz was willing to oppose evolution on the most inconclusive grounds, in this case geological "discoveries" that did not stand the test of time.[39]

Agassiz's students at the Museum of Comparative Zoology also felt the weight of their teacher's rejection of Darwin. The museum was for a time organized according to Cuvier's system of classification, a procedure that caused Jeffries Wyman to comment: "Agassiz is often inconsistent with himself in the application of his principles, and his own

[37] *Methods of Study in Natural History* (1863); *Geological Sketches* (1866); [with Mrs. Agassiz], *A Journal in Brazil* (1868). These books all contained articles that had appeared in the *Atlantic Monthly* during 1862-64 and 1866 (Vols. IX, X, XI, XII, XIII, XIV, XVIII). All of these articles comprised lectures at the Lowell Institute in 1862-63, 1864, and 1866. *Atlantic Monthly* articles of 1864 and 1866 appeared in book form as *Geological Sketches, Second Series* (1876).

[38] *A Journey in Brazil*, 398-441; Gray to Darwin, 27 Aug. 1866, Gray Papers.

[39] Lyell to Charles Bunbury, 3 Sept. 1866, in *Life . . . of Lyell*, II, 410; Darwin to Gray, 10 Sept. 1866, Historic Letter File, Gray Herbarium; James Geikie, *The Great Ice Age* . . . 3rd ed. (1901), p. 723.

students cannot make them work."[40] Agassiz, however, was convinced that with enough public support he would find evidence to refute Darwin. In 1863, for example, he initiated a world-wide search for fishes native to every known river system, convinced that the knowledge of geographical distribution thus gained would prove the multiple creation and permanence of species.[41] He was so certain of this that he assured public officials that the donation of state aid to the museum would make possible evidence to prove Darwin wrong.[42] But Agassiz was so immersed in the public campaign against evolution, in the administrative and organizational aspects of science, and in the effort to gather ever greater quantities of specimens in Cambridge, that he never applied himself to the exact study of even one group or series of specimens in the vast array of data at his command.

Acting under an impulse to make his view of nature understandable to all, Agassiz continued to isolate himself from intellectual interchange with fellow naturalists, preferring instead the comfortable society of worshipful lecture-hall audiences, cultivated laymen, and scientists not engaged in the study of natural history, where it was fruitless to try to oppose him. Since he avoided professional disputation his attacks on Darwin and evolution seemed all the more distasteful to colleagues who had once respected and admired him. Gray's comment to Darwin[43] revealed the degree to which Agassiz had divorced himself from any intellectual relationships premised on equality or mutual interchange with men who wanted an evaluation of Darwin's work to proceed from an objective appraisal of evidence.

Praise such as that of Oliver Wendell Holmes[44] could dispel the temporary frustrations of arguments over evolution. It could and did provide the basis for a public image of a dedicated naturalist that lasted for generations to come.

Yet Agassiz could not help but feel the passage of time, could not avoid the gnawing realization that ideas, events, and men were estab-

[40] Wyman to Dana, 15 Nov. 1863, Dana Papers.

[41] "Directions for Collecting Objects of Natural History" [May 1863], Agassiz Papers, Museum of Comparative Zoology.

[42] "Report of the Director" in *Report of the Trustees of the Museum* (1864), p. 10.

[43] "Agassiz is writing very maundering geology and zoology, and worse botany. . . . He is 'joined to his idols,' and I have no expectation that he will ever be of any more direct use in nat.[ural] history" (28 May 1863, Gray Papers).

[44] "I look with ever increasing admiration on the work you are performing for our civilization. It very rarely happens that the same person can take at once the largest and deepest scientific views and come down without apparent effort to the level of popular intelligence. This is what singularly gifts you for our country" (Holmes to Agassiz, 20 Oct. 1863, Agassiz Papers, Houghton Library).

lishing a new world of scientific inquiry that was being constructed without his participation. Thus the period from 1868 until his death in 1873 saw him make a valiant effort to understand the meaning and mechanisms of Darwinian evolution. Since he could not turn back the clock of scientific opinion, he undertook to fashion the means for a new and hopefully favorable relationship to Darwinian thought. Unfortunately, poor health impeded him, and the overpowering force of philosophical conviction once more prevented any acceptance of the evolution idea. Yet the fact that he made the effort, and the more moderate tone of his critique, suggest a changed attitude.

In July 1868 Agassiz wrote Darwin as the result of an inquiry that Darwin had made of Asa Gray, concerning sexual selection in fishes. Gray referred the problem to his Harvard colleague, and Agassiz informed Darwin that he knew no better way to contradict reports of ill-feeling on his part toward the English naturalist than by providing such information directly. Once this was done, Agassiz described his personal relationship to Darwin's work: "It is true that I am and have been from the beginning an uncompromising opponent of your views . . . it is equally true that I hold these views as mischievous because they lead to a looseness of argumentation which it has been the aim of science to avoid. . . . there is nothing in these feelings against yourself, as you have done original researches . . . and as to allowing my feelings to be the master of my judgement, I hope I shall never be guilty of such a mistake."[45] Darwin replied that he thought Agassiz "had formed so low an opinion" of his work that it would have been improper to ask information of him directly, so that this indication to the contrary pleased him greatly. "I have never for a moment doubted your kindness and generosity," Darwin wrote, "and I hope you will not think it presumptuous in me to say that when we met many years ago I felt for you the warmest admiration."[46]

Three years later, in 1871, after a long illness that had prevented any work, Agassiz read Darwin's recently published *Descent of Man* and took this occasion to deliver a series of lectures to his students at the museum. These lectures showed how new paleontological discoveries suggesting variation in ancient forms[47] had made him pause in his dogmatic refutation of Darwin. Darwin was a good observer, he told

[45] 22 July 1868, Museum Letter Books, IV, 169-170, Museum of Comparative Zoology.
[46] 19 Aug. 1868, Agassiz Papers, Houghton Library.
[47] Agassiz to Sir Phillip Grey Egerton, 9 Nov. 1870; to Gerard Krefft, 9 and 29 Nov. 1870; to Lord Eniskellen, 8 Dec. 1870; Museum Letter Books, VI, 6-10, 67-68, 104-105, Museum of Comparative Zoology.

students, and he felt the greatest admiration for the man "on account of his perfect honesty and love of truth and his willingness to face obloquy or misrepresentation when convinced his belief is right." It was as if the Agassiz of the present was praising Darwin for his courage in meeting the attacks of the Agassiz of the past. Since the facts of nature were known equally to Darwin and himself, Agassiz tried manfully to discover the real source of their disagreement. With remarkable candor, Agassiz admitted that his views, and those of any scientist, were influenced by personal experiences, historical ideas, religious convictions, habits, and intimate feelings. Agassiz affirmed that he accordingly made every effort to free himself from subjectivity by simply observing nature "like a sensitive statue," trying not to interpret facts while thus engaged but simply allowing them to "take whatever place they would in my mind and rest there as long as they chose and by and by, by some unconscious method they would work out." Darwin had observed nature in this way too, prior to 1859. But after that his work became marked by presupposition and argumentation, an understandable failure since "the English possessed this propensity for fanciful theories more than any other nation."

Once more the memories of former training and education rose to haunt Agassiz, and he talked of Darwin as if the Englishman were Oken or Lamarck and he was Cuvier. Scientists had to learn to let the weight of the evidence, and not preconception or argumentation, determine the truth. Even worse, "evolution appeals to a general disposition on the part of the public, that is the facility in which it can be indulged, for it demands no thought, no labor. Any man with wit and plausibility can argue and present it so that it will be popular." Darwin and evolution, then, were condemned for preconceived notions and popularization. Agassiz concluded his lecture with the injunction that scientists must "fairly, fully own up to the manner in which we do our work. It is a difficult thing to do. But science has a right to ask of us — How do you do it; and she will be benefitted by the answer."[48]

Agassiz apparently took his own command seriously, because late in 1871 he set out on a journey down the Atlantic shore of South America, through the Straits of Magellan, north through the Pacific to Central America, and thence to San Francisco. His purpose was to test new dredging equipment which might yield information of first importance. If marine life from the deepest waters of the oceans, living under condi-

[48] Anon. [class notes on evolution lectures by Agassiz], 10 and 17 Mar. 1871, Agassiz Papers, Museum of Comparative Zoology.

tions prevailing in the shallow seas that had covered the world in earliest times, could be compared with life in upper ocean levels it would be possible to determine whether any genetic relationships existed between such forms. This was an admirable purpose, but Agassiz had another significant intention that related to his assessment of evolution. He wrote his friend Carl Gegenbaur in Europe, pledging him to secrecy: "I wanted to study the whole Darwin theory free from all external influences and former surroundings. It was on a similar voyage that his present conceptions awakened in Darwin! I took along a few books so as to remain free from special studies; mainly Darwin's principal works, his Journal of Researches, Origin of Species, Domestic Animals, and Descent of Man."

Agassiz confessed to his friend that he had lived through many different periods of biological interpretation in his lifetime and had pondered the significance of these changing approaches. If an idea he had once believed in had been proved false, perhaps his present views would seem as incomprehensible to later generations as the teachings of *Naturphilosophie* seemed to him at the present time. He had determined, therefore, to cease his former dogmatic opposition to Darwin. "I no longer leap in with the fiery enthusiasm I would have exhibited some thirty years ago, but I first examine step by step, and I must admit that up to now I have not made any great progress in my conversion to the doctrine of evolution."[49] It was unfortunate that Agassiz found no physical evidence or intellectual opportunity to change the opinions of a lifetime in this journey. The dredging equipment failed to operate properly, and Agassiz's poor health did not permit the kind of mental ease he had hoped to enjoy on this ocean cruise.

It was understandable, then, that upon returning to Cambridge Agassiz continued in his traditional role of Darwin's leading opponent. In the fall of 1873 he delivered a series of lectures designed to expose the falsity of evolution, and decided to publish these addresses in the *Atlantic Monthly*. The first article, "Evolution and Permanence of Type," was Agassiz's last public statement. The article appeared in January 1874 (pp. 92-101), one month after the death of its author. Americans were thus able to read the scientific will and testament of Cuvier's last and most devoted student.

The passage of time had given Agassiz a respect for Darwin that he had not shown in 1859. He seemed to strive desperately to erase this

[49] 28 July 1872, Agassiz Papers, Houghton Library.

new appreciation from his assessment. Thus the carefully chosen words of this final analysis represented all facets of Agassiz's scientific personality. As an observer of nature he could not help admiring the "startling array of facts" Darwin had presented over the years. He was forced, at last and reluctantly, to grant Darwin credit for originality and a status equal to that he had reserved for himself and a few others. "Darwin has placed the subject on a different basis from that of all his predecessors, and has brought to the discussion a vast amount of well arranged information, a convincing cogency of argument . . ." (p. 94). Yet the disturbing parallel between Darwin and Oken seemed to be an oppressive weight preventing Agassiz from any fresh appreciation of evolution. Both men had "claimed that the key had been found to the whole system of organic life." Darwin, he admitted, had been unique in maintaining such an all-embracing view "not upon metaphysical ground but upon observation." These twistings and turnings of Agassiz's almost agonizing reappraisal of evolution demonstrated the unhappy combination that idealism, dedication to the sacredness of "physical fact," and isolation from professional intercourse had manufactured in Agassiz's mature intellect. Darwin had offered a "key" to explain the phenomena of natural history; but encompassing views were supposed to be "metaphysical" according to the experience derived from education in Germany. How was it possible to grant intellectual status to a theory grounded upon observation? It was thus gratifying when Darwin's interests led him to examine expressions of emotion in man and animals. "This last phase of the doctrine is its identification with metaphysics. . . . I can only rejoice that the discussion has taken this turn" (p. 95). But such sought-after reassurance did not obviate the impressive manner in which Darwin had dealt with "facts." A physical fact, after all, was "as sacred as a moral principle." Empiricism, however, learned faithfully from Cuvier, was supposed to furnish the ammunition that would destroy fanciful theories of "development." Now, Darwin piled fact upon fact to demonstrate by "cogency of argument" the key to understanding all natural history.

Agassiz was quite plainly a victim of the twin forces of his education and his self-assumed authority to designate the way in which nature should be investigated. When Darwin and his followers studied the facts of nature and arrived at objective conclusions that were at once synthetic and universal, Agassiz quite understandably felt alone, misunderstood, and the sole defender of a singular tradition. He could thus write in all honesty, "the science of Zoology . . . has furnished, since the beginning of the nineteenth century, an amount of startling and exciting information in which men have lost sight of the old landmarks" (p. 92). What was

a devoted student of Cuvier to do in such a situation? The only possible attitude was to rest upon the authority of "the old landmarks" and deny the transmutation of species, partly on idealistic grounds but much more intensively from "evidence." Hence Agassiz's final statement on evolution was more in the spirit of objective inquiry than anything he had ever written on the subject. But since he chose the grounds of disputation, he inevitably predetermined the outcome of his analysis. Thus admitting that "it is not my intention to take up categorically all the different points on which the modern theory of transmutation is based," he persisted in his own definition of what "evolution" meant. Demonstrating the indelible influence of his education, he identified evolution with "metamorphosis" and the ontogenetic transformations demonstrated by embryology. It was a foregone conclusion that such an identification "proved" that "all embryonic growth is a normal process of development, moving in regular cycles, returning always to the same starting-point, and leading always to the same end." It was an established fact, therefore, that "metamorphoses have all the constancy and invariability of other modes of embryonic growth, and have never been known to lead to any transition of one species into another" (p. 98).

Agassiz did try, in half-hearted fashion, to assess the primary mechanisms of Darwinian analysis. But here too the same difficulties plagued him as at an earlier date. Agassiz never really found the freedom to keep pace with the internal progress of his science. The result was that he was able to embrace a consistent anti-Darwinian attitude of mind. Perhaps this was the fundamental explanation for his failure to investigate and assess the evidence for evolution on objective grounds after 1859. Had he followed the path of his contemporaries, this might have meant the need to disavow all personal and intellectual influences that had been such sources of strength in the great days of accomplisment he had known before 1859. Living in an age of intellectual transition, Agassiz cast his lot with the certitude provided by the past. This assurance seemed as reliable as the permanence that he saw in all nature. Although such a choice meant that Agassiz was unable to appreciate or make use of the Darwinian insight, it was an inevitable decision in the light of his powerful belief in his own authority. No compromise or reversal was possible under such conditions. The result was the consistent affirmation of a philosophy of nature that served the valuable purpose of illuminating ideas and issues in an extremely significant period of scientific and philosophical dispute.

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REVIEW ARTICLES

Evolution: Two Books and Some Darwin Marginalia*

> . . . *die Geschichte der Natur ist nur die Geschichte fortschreitender Siege des Geistes uber den Stoff. Das ist der Grundgedanke der Schopfung . . .*
> K. E. von Baer (1833)

THE IDEA OF ORGANIC EVOLUTION by means of natural selection was announced jointly by Charles Robert Darwin and Alfred Russel Wallace at a meeting of the Linnean Society of London held on 1 July 1858. Darwin's summary account of his ideas and the evidence for them, still lacking full documentation, was published on 24 November 1859 as the book now called in brief the *Origin of Species*. The first edition of 1,250 copies was sold out on the first day. Interest in the work was maintained at a high level, partly, no doubt from the fortunate chance that T. H. Huxley's (unsigned, but later acknowledged) brilliantly perceptive review was published in *The Times* on 26 December 1859, the day of all days when the literate public had sufficient leisure to read with attention whilst recovering from their Christmas excesses. The second printing of 3,000 copies was issued on 7 January 1860, bearing the legend "5th thousand," and sold out almost as quickly.

The centenaries of these two events are now upon us; they have already precipitated a shower of celebratory publications, two of which are the subject of this review. Both are good specimens of their kind.

There has been so much written concerning Darwin's achievement and its impact upon practically all aspects of human life and thought that it is incumbent upon the reviewer to justify any additional comment of his own. The publication of the source documents for assessing Darwin's life and thought, begun by his son Francis in 1887 with the three-volume *Life and*

* *A Hundred Years of Evolution*, by G. S. Carter; pp. x + 206. Sidgwick and Jackson: London, 1957, 21s.; Macmillan: New York, 1957, $3.75.
Darwin's Century: Evolution and the Men Who Discovered It, by Loren Eiseley; pp. xvii + 378. Doubleday: New York, 1958, $5.00; Gallancz: London, 1959, 21s.

Letters, still continues; Darwin's granddaughter, Lady Barlow, has recently presented most valuable editions of the *Beagle* diary and notebooks and the full text of Darwin's *Autobiography*. Since the publication of Dr. A. O. Lovejoy's *The Great Chain of Being: A Study of the History of an Idea* in 1936, there has developed in English-speaking countries an interest in the history of science and philosophy which has resulted in an extensive range of specialist studies bearing on the wider issues of evolution. It is therefore something of a shock to realise after a most cursory examination that there is still primary source material to be edited and published among the Darwin papers housed in Cambridge University Library and at Down House. The general public needs guidance through this massive and growing documentation; but guidance necessitates selection, and selection omissions. Fleeting fashions condition the subject matter when an author writes to sell to the general public. It is, therefore, a most instructive exercise to survey, giving attention to their content and handling, the existing library of popular expositions of Darwinism. Today the need for efficient communication between scientist and the rest of educable mankind is perhaps more pressing than at any other time in recorded history. The language of the scientist is full of shop-soiled and technically specialised terms which carry for the initiate overtones of meaning and subtle resonances lost to anyone who has not seen the need to subject himself to the dedicated drudgery of a scientific training.

In Dr. Carter's book the Darwinian achievement is set in a historical perspective extending from the eighteenth century, with its revolution in geological thought begun by Hutton and expounded by Playfair, to the present day, with its successful demonstrations of the operation of natural selection. From his background of physiological research in a variety of tropical habitats, it is fitting that Dr. Carter should place particular emphasis on ecology. He outlines the impact of the Darwinian theory and the rich progeny of productive research it has promoted. The treatment, though brief, is conspicuously fair; rather too impersonal perhaps, but as an outline guide for the interested layman it could hardly be bettered. The reader might have been given more references to important source material, and more guidance to further general study than the rather sparse footnotes convey. The book is, however, written with an economy and elegance to be expected from an author with such long teaching experience.

Dr. Eiseley's book is most stimulating reading. It is a product of enthusiastic, sporadic, but intense scholarship. The author writes with percipient insight passages of memorable quality, while at other times, fortunately rarer, the argument is held up by an hypertrophy of words which can be truly baffling. On page fifty-two, for example, we read, "If, now, in retrospect, we cast an eye backward . . ." The intention is no doubt poetic, but the achievement suggests illiteracy.

Dr. Eiseley quotes from a number of unpublished theses submitted at universities in the United States and he is splendidly at ease with the voluminous literature in the English language, but even here an occasional remark gives the reader pause. Wells' "An Account / of a female / of the White Race of Mankind / Part of whose skin / Resembles that of a negro; /

with / Some observations / on the / causes of the differences in colour and form / between / the white and negro races of men" (this is the correct version of the title and differs from Dr. Eiseley's), though brief in itself (fourteen pages), was not published in a "little volume" as Dr. Eiseley suggests, but at the end of an octavo book of 439 pages and an introductory autobiography of seventy-four pages. (This introductory autobiography is incidentally surely worth reprinting.) It is a pity that Dr. Eiseley does not quote Lamarck and Buffon in the original. If Buffon has to be quoted in translation there is a strong case for the splendidly vivid versions given by Samuel Butler in his *Evolution Old and New* rather than the pedestrian versions used by Dr. Eiseley. Radl's *Geschichte der Biologischen Theorien* should never even be consulted in the mutilated, mistranslated version of 1930; it is so much more profitable to go back to the original edition in two volumes (1905 and 1909). Dr. Eiseley should surely find a place to mention Émile Guyénot's classic account *Les Sciences de la vie aux XVII^e^ et XVIII^e^ siècles: l'idée d'évolution*, published in 1941 by Albin Michel. As a final criticism Dr. Eiseley, or his publisher, is addicted to that infuriating and time-consuming usage *op. cit.* These minor points are raised because it is of the utmost importance that Dr. Eiseley's book should go on to new editions.

Dr. Eiseley is a fossil hunter, naturalist, and latterly anthropologist. He writes so well that the reader is transported with him in the company of the young Darwin collecting fossils in the Pampas and scrambling over the ghastly slag heaps of the Galapagos Islands with their tortoises, lizards, and fantastic birds, each species subtly differing from one island to the other. This sharing of the naturalist's experience is essential if the general reader is to gain even an inkling of Darwin's greatness. Dr. Eiseley has gone over the ground with manifest care and he contributes largely to an eventual, profitable reassessment of the available evidence.

He is particularly enlightening when he discusses the influence Lyell's *Principles of Geology* exerted upon the development of Darwin's ideas of evolution. This indebtedness was acknowledged by Darwin in his old age during gossips about geology with Professor Judd. The second volume of Lyell was published in January 1832 (Dr. Carter says the whole work was published in 1830, the year the first volume – which Professor Henslow persuaded Darwin to take with him on the *Beagle* – appeared), and Darwin's copy, received at Montevideo 26 October 1832, had to wait for a few days until newspapers and letters from home had been read. This volume, inscribed "M:Video. Novem 1832" is now preserved at Down House. Darwin read it with close attention, though some of the index pages are still uncut, but, unlike the first volume, it has very few annotations, and those only in passages relating to the formation of coral reefs. Dr. Eiseley quotes Lyell's comments on the species problem from the third edition, published May 1834 in four duodecimo volumes reset from the preceding three-volume octavo editions. This renders collation with the first edition somewhat laborious, but, nevertheless, this has been done. All the passages quoted by Dr. Eiseley occur in the second volume of the first edition and presumably were seen by Darwin while on the *Beagle* voyage. There does not, however, seem to have been an

immediate reaction, and the full response of Darwin to Lyell's studies had to await his return to England when the *Beagle* reached Falmouth on Sunday evening, 2 October 1836. Darwin writes to W. D. Fox on 6 November 1836 concerning Lyell, "I have seen him several times, and feel inclined to like him very much. You cannot imagine how good naturedly he entered into all my plans."

Lyell had been trained in law, and he gives summaries at the end of all his chapters modelled on the style of an "opinion." He also revised every edition of his work, with a result that no two editions show the same pagination. The fifth edition of the *Principles* was published March 1837 in four duodecimo volumes; Darwin was probably given his copy by Lyell himself – a letter (9 Aug. 1838) survives from Darwin thanking him for a gift of the "first copy distributed" of the *Elements*. It is most interesting to observe that this copy of the fifth edition is most heavily annotated by Darwin in those passages where Dr. Eiseley sees most sign of the influence of Lyell.

It is probable that these annotations in the fifth edition were written between March and July of 1837, when Darwin, already a convinced evolutionist, began the first of his many notebooks on the "Transmutation of Species." The annotations do not seem to have been noted hitherto, and I give herewith a sample of the comments elicited by Lyell's recapitulation of his reasons "in regard to the reality of *species* in nature." Lyell's text is given in full, with Darwin's annotations quoted within brackets.

First, That there is a capacity in all species to accommodate themselves, to a certain extent, to a change of external circumstances, this extent varying greatly according to the species.

2ndly. When the change of situation which they can endure is great ["or small"], it is usually attended by some modifications of the form, colour, size, structure, or other particulars; but the mutations thus superinduced are governed by constant laws, and the capability of so varying forms part of the permanent specific character. [There is a line in the margin to draw attention to this last clause.]

3dly. Some acquired peculiarities of form, structure, and instinct, are transmissible to the offspring ["with no tendency to go back"]; but these consist of such qualities and attributes only as are intimately related to the normal wants and propensities of the species ["respect to changes superinduced in short period"].

4thly. The entire variation from the original type, which any given kind of change can produce, may usually be effected in a brief period of time, after which no farther deviation can be obtained by continuing to alter the circumstances, though ever so gradually, – indefinite divergence, either in the way of improvement or deterioration [the last three words underlined by Darwin] being prevented, and the least possible excess beyond defined limits being fatal to the existence of the individual ["if this were *true* adois *theory*"].

5thly. The intermixture of distinct species is guarded against by the aversion of the individuals composing them to sexual union, or by the sterility of the mule offspring. It does not appear that true hybrid races have ever been perpetuated for several generations, even by the assistance of man; for the cases usually cited relate to the crossing of mules with individuals of pure species, and not to the intermixture of hybrid with hybrid.

6thly. From the above considerations, it appears that species have a real existence in nature, and that each is endowed, at the time of its creation, with the attributes and organization by which it is now distinguished.

The second volume ends here, leaving blank a half sheet and a facing sheet which are covered with more notes in Darwin's handwriting. I transcribe two of these which seem to relate to the preceding.

The great difficult [*sic*] appears, that though some animals long domesticated change not indefinite (Do we know this), but most domesticated animals are hereditary monsters. Yet we should have expected some race which would have showed a slight repugnance *to breed* with our animals.

2° The changes apparently being rapidly superinduced, in domesticated animals N.B. The very character is character being hereditary, & as we know can give forms not hereditary, some that are we might expect gradation.

In his diary of the most important events in his life Darwin writes for 1837, "In July opened first note-Book on 'Transmutation of Species'. – Had been greatly struck from about month of previous March on character of S. American fossils – & species on Galapagos Archipelago. – These facts origin (especially latter) of all my views." In the light of the evidence here submitted it is permissible to correlate the reading of Lyell's fifth edition with this event, for it would be in March that a copy would first be available to him. Incidentally the passage quoted above occurs word for word in both first and fifth editions. Why is the first edition not annotated?

The three essential minimal hypotheses for present day Darwinian theory are: the occurrence of variation in nature; the selection of more favoured variants in a particular habitat; and the preservation of these variants intact for hereditary transmission to their progeny without dilution by a process of blending inheritance. The response by Darwin to the passage quoted above shows that as early as March 1837 his grasp of the essentials of his theory, though probably intuitive, was complete. His picking up these particular points in Lyell's "opinion" for comment can be interpreted in no other way. Darwin is plainly unwilling to accept any limitation of the ultimate extent of any change which might result from the selection process, as his comment, "if this were *true* adios *theory*," shows. He is not accepting or even contemplating the implications of blending inheritance at this date, or he would not have added the gloss "with no tendency to go back" and the note "the very character is character being hereditary." Darwin in his drafts of 1842 and 1844 was to sophisticate this almost mathematical intuition as the weight of accumulated evidence and the caution bred of the advice from friends led him to weaken his argument. It is notorious that this process continued further in the successive editions of the *Origin*, and Eisley makes this point extremely well in his book.

I have asked above, "Why is the first edition not annotated?" and perhaps I may be permitted some speculation concerning this problem. The first, most simple, notion is that Darwin's copy was sent back to England fairly soon after receipt and reading, packed in a batch of specimens. The despatch of material from time to time was a consequence of the growing bulk of his collections and the exiguous nature of the accommodation available to him on the *Beagle*. This notion must be rejected since the only important annotation in the margin of Volume II is the single word "Galapagos" on page 294 opposite Lyell's passage concerning the peculiar fact that the leeward side of

a coral fringing reef is much lower than the windward side. Lyell suggests subsidence by earthquakes as a possible explanation, and we know that a regular subsidence is an essential component in Darwin's theory concerning the origin of coral reefs. It seems inconceivable that in 1832 he was aware even of the existence of the Galapagos Islands – they were not to be reached until mid-September 1835. The book must therefore have remained with him at least until that decisive phase of the voyage.

The only other alternative is that Darwin did not react to the text of Volume II because in some way he was disappointed by Lyell's withdrawal from the advanced viewpoint which permeates the first volume and which had caused Darwin to forge ahead with his own speculations, stimulated as he was by his finds of giant fossil mammal remains in between marine shell-bearing strata at Bahia Blanco. This event was argued by Professor Judd* as the first origin of Darwin's doubts as to the fixity of species in nature, and I find the evidence there surveyed quite convincing. Lyell had delayed the final writing of this volume and had been subject to pressure from friends, as he confessed later in life in his letter to Whewell of 7 March 1837 which reads "you remember what Herschel said in his letter to me. If I had stated as plainly as he has done the possibility of the introduction or origination of fresh species being a natural, in contradistinction to a miraculous process, I should have raised a host of prejudices against me, which are unfortunately opposed at every step to any philosopher who attempts to address the public on these mysterious subjects." I think that Lyell's evident caution acted as a cold douche to the young, diffident, and inexperienced Darwin and caused to set in a period of endless oscillations of doubt and difficulty of which he speaks in his letter of 19 June 1863 addressed to George Bentham.

The fact that your reviewer has been stimulated to add to the results of the detective enthusiasm shown by Dr. Eiseley is an earnest of the compelling quality of his work at its best. It is clear that Darwinian scholarship has much to gain from this further example of the "extraordinary kindness of the Yankeys," which was Darwin's comment in one of his small notebooks on the gift by the crew of an American whaler of three casks of water and "a bucket of Onions." The occasion was during a collecting trip on the Galapagos Islands when things became critical for the party as their drinking water had become contaminated with sea water. I can do no better than end with a further quotation from the *Beagle* diary. "Several times during the Voyage Americans have showed themselves at least as obliging, if not more so, than any of our Countrymen would have been. Their liberality moreover has always been offered in a most hearty manner. If their prejudices against the English are as strong as ours against the Americans, they forget & smother them in an admirable manner."

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* John W. Judd, "Charles Darwin's Earliest Doubts Concerning the Immutability of Species," *Nature*, LXXXVIII (1911-12), 8-12.

The Logic of Evolution: Some Reconsiderations*

IT WOULD BE UNUSUAL, I should think, to be a Victorian scholar in September 1959 without having come across a Darwin Celebration or Commemoration of some sort. And at first it might seem odd that these are generally conducted not by biologists but by members of history, philosophy, and English departments. Such sponsorship will perhaps seem least strange, however, to readers of this journal, where the interrelation of "fields" of knowledge is frequently stressed. Certainly a "journal of the Humanities, Arts, and Sciences" has a responsibility to recognize this interrelation in the case of Darwin, whose cross-disciplinary influence has been so profound.

"Thought makes the whole dignity of man": Pascal's generalization is unexceptionable and might well stand as epigraph for students of the humanities. Reflecting on Darwin's work reminds us, though, that the statement needs at least one qualification: we tend to think within whatever framework our age suggests. Darwin was substantially responsible for three important modifications in our own framework of thought. Put very briefly, they are as follows. First, Darwin gave early scientific authority to the tendency to think in terms of wholes and continuities rather than discrete parts and rigidities. Second, the

* *The Origin of Species*, by Charles Darwin, with an introduction by Sir Julian Huxley; pp. xv + 480. Mentor Books: New York, 1958, $.50. Referred to in this article as *Origin*.

The Life and Letters of Charles Darwin, edited by Francis Darwin, with a foreword by George Gaylord Simpson; 2 vols., pp. xxii + 588 and 562. Basic Books: New York, 1959, $10.00. Referred to as *Life*.

Forerunners of Darwin, 1745-1859, edited by Bentley Glass, Owsei Temkin, and William Straus, Jr.; pp. viii + 471. Johns Hopkins Press: Baltimore, 1959, $6.50. Referred to as *Forerunners*.

Darwin and the Darwinian Revolution, by Gertrude Himmelfarb; pp. 480. Doubleday: New York, 1959, $5.95; Chatto and Windus: London, 1959, 42s. Referred to as Himmelfarb.

Charles Darwin: Evolution and Natural Selection, edited by Bert James Loewenberg; pp. 438. Beacon: Boston, 1959, $2.25. Referred to as Loewenberg.

Just Before Darwin: Robert Chambers and "Vestiges," by Milton Millhauser; pp. x + 246. Wesleyan University Press: Middletown, Conn., 1959, $4.50. Referred to as Millhauser.

Quotations are cited entirely from books under review. It will be clear from context when the reference is to a secondary source.

evolutionary orientation stresses context, complexity, and tension. And third, Darwin's discovery of natural selection spelled the end of the naturalistically teleological view of man and his world. All three of these are of great importance to students of the humanities, for our whole attitude toward life is conditioned by them; it is therefore difficult indeed to write about Darwin without making reference to this fact.

I

The books listed in my headnote are all consciously centenary publications and in one way or another they all represent philosophical outcroppings of evolution. Three of them reprint primary materials. The Mentor *Origin* (the sixth edition) will be especially welcome to teachers who want a text that is inexpensive, and it has the additional virtue of a brief historical sketch and evaluation by Sir Julian Huxley. This introduction is largely given to a defense of Darwin's originality and his scientific significance today and reflects, even in its brevity, Huxley's intimacy with his subject. If it has any shortcoming, it is that of overenthusiasm: when Huxley insists, for instance, that natural selection is now "firmly and finally established as the sole agency of major evolutionary change" (p. xiii), one wonders what meaning that word "finally" can have, in this context, to a scientist's mind. Another reprint is of Darwin's *Life and Letters*, long unavailable. George Gaylord Simpson's new foreword tries – in rather the same terms as Huxley's – to evaluate Darwin's contribution and (also like Huxley's) performs a useful service for the lay reader in pointing out the major modifications by which natural selection has become the foundation for "Neo-Darwinism."

Mr. Loewenberg's book is an anthology of Darwiniana, presenting under one cover the quintessence of Darwinism. Mr. Loewenberg has made an astute selection of passages (from the autobiography, the *Beagle Journal*, the *Life and Letters*, the Linnean Society papers, the *Origin* and *Descent*, and elsewhere); his introduction, though short, fills in the background fairly well and makes some key points as it goes along (see the comments on Darwin's objectives, on p. 12, and the description of Darwin's achievements, p. 21); and there are a number of illustrations. The book should be useful in both graduate and undergraduate courses dealing with the nineteenth century.

Of the three secondary books, Gertrude Himmelfarb's *Darwin and the Darwinian Revolution* claims most for itself. It is a full-length study, historical, biographical, and analytical. Miss Himmelfarb is an excellent reporter and, having delved into all sorts of Darwiniana, omits little. The story of Darwin's life and work is laid out in great detail and (although in 480 pages) compactly. Often tactfully, too: all of the biographical chestnuts are there, for instance, but they are not raked out of the fire with any to-do; the account of Darwin's illness is naturally there, but one is grateful to Miss Himmelfarb for not finding it sinister or neurotic. By a judicious selection of quotations she has kept the book lively; by mastery of her multitudinous materials and of the art of synthesis she has made this big book readable – even graceful. It is the analytical passages by which Miss Himmelfarb makes her greatest claim, how-

ever, and it is here that, it seems to me, she often fails. For if Sir Julian's introduction is perhaps guilty of overenthusiasm, Miss Himmelfarb's analyses seem to be grounded upon a hostility for Darwin's work that is so deeply felt as to make of her discussions – frequently – a kind of learned carping. A charge this serious needs support; I shall return to it after briefly describing the other two secondary books.

Forerunners of Darwin tells a familiar story – that Darwin did not "invent" evolution – but does so on a scale so large and with a reach so broad that it hardly seems the same tale. The book is a collection of essays by members of the History of Ideas Club at Johns Hopkins University and by guests of that club. Of the shortcomings that are the almost inevitable concomitants of this sort of genesis, the chief is a tendency, as a book, to seem scattered. (Mr. Glass frankly calls its growth "haphazard.") Some of the essays, furthermore, range so far afield that one might, I think, be considerably interested in Darwin and evolution without being much interested in the essays. But unity is not the only virtue in a book; and *Forerunners* is about the forerunners, after all, not about Darwin. The strength of the volume lies in its individual essays and here the performance is generally satisfying. Readers of *Victorian Studies* will no doubt be chiefly interested in the section on the nineteenth century, which occupies nearly half of the book. I was much taken with C. C. Gillispie's accurate "placing" of Lamarck and Darwin in the history of science, and with Arthur O. Lovejoy's revealing essay on Robert Chambers. Through the latter there runs, however, some of that tendency for *Forerunners* to stress "background" so much that the "foreground" is lost.

Erasmus Darwin, Lamarck, and Robert Chambers (not to mention the other "predecessors" of Darwin, going back to the Greeks) were hardly considered great prophets of science in 1858. After the publication and general acceptance of the *Origin*, however, they were looked back to as significant "forerunners" of Darwin, and some of Darwin's critics (notably Samuel Butler) tried to discredit – or at least deflate – Darwinism by pointing to its unoriginality. Mr. Millhauser's *Just Before Darwin* does not indulge in this sport, notwithstanding the title. Robert Chambers is, in fact, a subject having his own considerable interest, and Mr. Millhauser tells his story with real skill; it is a vignette of history now, but a vignette made up of what once seemed cataclysmic events. It is to Mr. Millhauser's credit that he claims just enough for Chambers, and not too much, as a precursor of Darwin. In a perceptive chapter, "In the Air," he makes some valuable distinctions with respect to the history of evolution (see especially his remarks on the "two histories" of evolution, pp. 59-60, and on its "two publics," p. 81) and Chambers' place in it, and concludes that "In a work like the *Vestiges* (and this would hold for *The Origin of Species* too) the author's contribution is not the absolute and parthenogenetic conception of the idea. It is the recognition of its importance, its relevance and scope and probable validity; it is the sifting of materials, the marshalling of evidence, the construction of an argument; it is insight, selection, organization, interpretation" (p. 84). As to these, Mr. Millhauser says, Chambers did "his share." But Millhauser never overestimates that share; he has accomplished that difficult thing in biographical writing, proper perspective.

II

Perspective is not everyone's forte, though, and there is a certain tendency, in some scholarly discussions of Darwin's relation to his forbears, to extreme statement: to imply that Darwin was only a creature, not a creator, of his time, a sort of historical accident. Thus Miss Himmelfarb, for instance, is occasionally given, in her more iconoclastic moods, to disparaging the *Origin*: "The observations on which it was based were largely familiar, the terms of the problem had been stated, even the crucial idea had been in circulation for half a century" (p. 423). She quotes Samuel Butler's classic of misunderstanding, "Buffon planted, Erasmus Darwin and Lamarck watered, but it was Mr. Darwin who said 'That fruit is ripe,' and shook it into his lap" (p. 423). For what Darwin's contemporaries experienced when they read the *Origin*, Miss Himmelfarb holds, "was not the shock of discovery but rather the shock of recognition. They were so quickly converted because there was little to be converted to" (p. 423). She even goes so far in this direction as to resuscitate Lamarck as being scientifically respectable, on the sanguine grounds that "*some* of the experiments which were presumed to be the definitive disproof of his theory are, in fact, indecisive" (p. 176; italics mine). All of this has a certain correspondence to truth; but it is badly assessed, its significance grotesquely inflated.

It is only natural to expect *Forerunners of Darwin* to make the strongest claims for Darwin's predecessors; and so it sometimes does. Some of the essays, however, do not touch upon this point at all, and others touch upon it with proper tact. The only essay with which I would quarrel seriously on this count is one by Mr. Lovejoy, an essay dating in part from 1909 (the nadir of Darwin's reputation, it will be noted) and extensively revised and supplemented since. "The Argument for Organic Evolution before the *Origin of Species*, 1830-1858" is a vindication of certain of Darwin's predecessors in the history of evolutionary thought. It focuses on the *Vestiges* and particularly on the ways in which it anticipated the kinds of evidence given in the *Origin*. Mr. Lovejoy's case for Chambers is a strong one: he traces in the *Vestiges* the arguments from sequence of type in paleontology, from persistent types, from the sterility of hybrids, from rudimentary organs, from "unity of type," and from comparative embryology.

It is somehow comforting to read Mr. Lovejoy's (and Mr. Millhauser's) demonstrations that Chambers was not such a poor fool as he has sometimes been made out. But when he concludes by recommending A. W. Benn's words (of 1878), "Hardly any advance has since been made on Chambers' general arguments, which at the time they appeared would have been accepted as convincing, but for theological truculence and scientific timidity" (p. 414), one is given pause. There was certainly nothing of "theological truculence" in either Darwin or Huxley, and it could hardly have been "timidity" that prompted Darwin to inscribe on a margin of his own copy of *Vestiges*, "the idea of a fish passing into a reptile, monstrous," or of speaking of it, in a private letter, as an "absurd though clever work." As for Huxley, the idea of "timidity" repressing him on any subject does not recommend itself.

Why, then, in view of Mr. Lovejoy's evidence, does evolution as a

scientific fact date from 1859, not 1844? The answers are various, but they can be simply summed up: Chambers did not compel assent, Darwin did. First of all, the *Vestiges*, Huxley said, "irritated me by the prodigious ignorance and thoroughly unscientific habit of mind manifested by the writer" (*Forerunners*, p. 359). The author, he said, knew no more science than "what might be picked up by reading 'Chambers's Journal' . . ." (Millhauser, p. 149). Later editions of the *Vestiges* corrected some of the errors of the first, and Darwin publicly noted this "improvement" (*Origin*, p. 20); but Chambers' too-frequent eccentricities, credulity, and mistakes vitiated, for rigorously scientific minds, the force of his argument.

Second, Chambers, although his "evidences" were similar to Darwin's, had no such array of them as Darwin did. Wallace, it is interesting to note, thought *Vestiges* "ingenious," but added some significant qualifications: Chambers' hypothesis "remains to be proved," he said, "*by more facts*"; and speaking of Chambers he added, later in life, "I firmly believed *a full and careful study of the facts* of nature would *ultimately lead* to the solution of the mystery" (*Forerunners*, p. 362; my italics). One is reminded of Darwin's distaste for Herbert Spencer's deductive approach to evolution: "His conclusions never convince me; and over and over again I have said to myself, after reading one of his descriptions, 'Here would be a fine subject for half a dozen years' work'" (Himmelfarb, p. 218).

Third, one must take sharp exception to Mr. Lovejoy's point that the discovery of natural selection was not a necessarily persuasive point (*Forerunners*, p. 363). Darwin and Huxley both insisted (*Origin*, p. 28; *Forerunners*, p. 359) upon the necessity of having a mechanism and indeed it is the mechanism that is at the very roots of Darwin's research and made the great difference between theory and conjecture. Chambers, of course, like the other forerunners, had no explanation so satisfactory.

Finally, Mr. Lovejoy and most other scholars rightly insist, as Darwin did, upon the evolutionary importance of Lyell's work: it extended the geological time-scale and established uniformitarianism as a methodological principle. This and other predecessors' investigations, they say, "prepared the way" for Darwin. And so they did. But Lyell himself, it must be remembered, did not come easily to evolution, and in fact Lyell's work is most accurately called only a necessary condition, not a sufficient condition, of Darwin's. Preparing the way is not making the journey. Mr. Simpson makes this point vigorously:

> Several recent studies of Darwin have stressed the fact that neither the idea of organic evolution in general nor that of natural selection in particular was completely new when he wrote *The Origin of Species*. Indeed, one of the centennial studies devotes such lengthy and detailed attention to the background of Darwin's thought that it seems almost determined to obliterate the foreground. . . . Yet it is naïve to proclaim as a "discovery" that Darwin did not live in a vacuum and that he resembled everyone else who ever lived in that his ideas had origins. In fact, despite Darwin's plain debt to others, his thought was unusually original. He would still be a great historical figure even if that were not true, because it is a historical fact that general acceptance of evolution by biologists stems from him, that it was he and no other who did open wide this door to a new world. (*Life*, I, viii)

There is a properly durable nineteenth-century epigram which is attributed now to one scientist, now to another: that it is the fate of all great discoveries to pass through three stages, "the first, in which people said, 'It is absurd'; the second, 'It is contrary to the Bible'; and the third, 'We always knew it was so' . . ." (Himmelfarb, p. 290). It seems that for some Darwin's work has never detached itself from this third stage, and this is a pity. Given another century, perhaps, there will be a fourth stage in which references to "in the air" are unanimously modified to read, "We always suspected it was so – but we never *knew* it until Darwin demonstrated it."

III

We must now return to Miss Himmelfarb's book because the importance it lays claim to makes a searching look necessary. "Many of the conventional ideas about science in general and Darwinism in particular," Miss Himmelfarb writes in her Introduction, "have to be discarded . . .," and a publisher's blurb, carrying this tone further, calls this "most certainly" the "definitive" work on Darwin, "a book which overturns the established appraisals of Darwinism." I must confess that this sort of language tends to make me uneasy in advance. And I must assume that it does *not* refer to Miss Himmelfarb's small-arms fire at previous biographers ("those biographers who . . . ," "his biographers, who have assumed . . . ," "Most biographers, carried away by . . ."). For "overturning" one needs more substantial blasts, and these Miss Himmelfarb has attempted to ignite chiefly in chapters fifteen ("The Argument of the *Origin*"), sixteen ("Mechanism and Teleology"), and twenty ("The Conservative Revolution").

About this "overturning" there is a great deal to be said. I shall be as brief as I can, commenting upon only four of the most important issues.

The first of the four concerns the general "reputation" of natural selection. A great deal of Miss Himmelfarb's analysis is devoted to the proposition that natural selection is not a proper explanation of the mechanics of evolution. This, if true, would be overturning, indeed; but her evidence is unconvincing. She relies, in part, upon the testimony of "authorities" (pp. 419-421). These, however, are spotty and, for the most part, badly dated (she therefore fills them out with "several scientists," "others," "there are many," etc.). Miss Himmelfarb, in choosing her authorities, overlooks what scientists call the "negative instances" – that is, she ignores those scientists who do not agree with her. The procedure is hardly justifiable, since, however much some scientists may complain that natural selection is not "predictive" and that it is not very useful as a "tool" in investigation, still the Neo-Darwinians (to whom Miss Himmelfarb refers in passing rather as a curious, opportunistic clique [pp. 312-313]) constitute in fact the great majority of biologists.

To refer, in rebuttal, only to those "authorities" writing in the books under review, one finds full agreement on the validity of natural selection: "Recently [natural selection] has been fully rehabilitated and is now recognized by most evolutionists as the effective directional (nonrandom) element

in evolution" (Simpson, *Life*, I, xi). "The hypothesis of natural selection demonstrated how evolution operated" (Loewenberg, p. 12). "The theory of natural selection is so widely taken for granted that its magnitude is not on the face of it apparent" (Gillispie in *Forerunners*, p. 282). Sir Julian's resounding testimonial has already been cited; it is his presentation of the "reputation" of natural selection (its apotheosis in the seventies, eighties, and early nineties, its decline from about 1895 to about 1925, and its subsequent rehabilitation) that is one of the useful features of his short essay, along with his terse description of Neo-Darwinism: "the modern theory of gradual transformation operated by natural selection on a Mendelian genetic outfit of self-reproducing and self-varying genes . . ." (p. xiii). The truth is that far from being discredited, as Miss Himmelfarb says, natural selection has rather been supplemented by Mendelian genetics to produce a new, hybrid mechanism superior to, but based upon, the original one.

The second point on which I must take issue with Miss Himmelfarb is her curiously contradictory time-perspective on Darwin's work. The first lines of her text are a scholarly caveat on historical judgments and she refers more than once (see, *e.g.*, pp. 59 and 433, n. 5) to the necessity for seeing a historical event in proper perspective. Yet in no way is the status of natural selection treated less fairly than in her mishandling of its historical development. She will, for instance, elaborate upon points of biological ignorance in 1859, and then conclude, despite more recent investigations which have substantiated Darwin, that natural selection is *at present* on shaky ground. The fact that Darwin could cite no paleontological form-series as satisfactory as that of the horse, say, apparently strikes her as something for which he is more to be censured than pitied — and she sees fit to put her commentary upon this in the present tense: "Geology, however, *has been* notably unforthcoming, and, instead of being the chief support of Darwin's theory, it *is* one of its most serious weaknesses" (p. 314; my italics). Similarly, when she calls the shortness of the geological time-scale in Darwin's day "the most vulnerable part of Darwin's case" (p. 315), she makes it sound as though it is still vulnerable; she does not bother to mention more recent estimations of the age of the earth.

If this sort of boggling is a source of frustration to the reader, the reverse tactic is even more so. Darwin frankly admitted that "much remains obscure" (*Origin*, p. 30) in his theory, and in the *Descent* he therefore floundered about trying to supplement natural selection. He "needed" a science of genetics, but did not have it. Miss Himmelfarb has it, however, and uses it not only (properly) in discussing Darwin's ill-starred "pangenesis" but also as another means of keeping natural selection at arm's length. Mendelian genetics, she says,

> solved the problem of the origin of variations only at the expense, it has been argued, of Darwin's theory of the origin of species. As genetics made pangenesis obsolete, so, in the opinion of some, it made natural selection superfluous.
>
> Genetics may be thought to supersede natural selection by making the explanation of the origin of variations do service for the origin of species. (p. 311)

Genetics "may" be thought to supersede natural selection, of course, but as a

matter of fact few biologists do think so. In order to favor this (unfootnoted) position, Miss Himmelfarb simply shrugs off the work of the Neo-Darwinians (pp. 312-313). This is at the least a commitment to scientific isolationism, and furthermore it again violates Miss Himmelfarb's own announced historical principles. She had been using Darwin's nineteenth-century ignorance to attack twentieth-century Darwinism; now she applies her twentieth-century knowledge to nineteenth-century conditions in order to continue the attack.

My third objection is to Miss Himmelfarb's conception of scientific proof. The question of scientific proof is not a simple one; Miss Himmelfarb has not satisfactorily come to terms with the nature of various kinds of proof, and certain of her assaults on Darwin's evidence for natural selection reflect this. For instance, she quotes Whewell approvingly: "it is assumed that the mere possibility of imagining a series of steps of transition from one condition of organs to another, is to be accepted as a reason for believing that such transition has taken place" (p. 316). And she adds that Darwin's theory was, as he said, " 'rendered in some degree probable' by one set of facts and could be tested and confirmed by another — among which he included the geological succession of organic beings. It was because it 'explained' both these bodies of facts that it was removed from the status of mere hypothesis and elevated to the rank of a 'well-grounded theory' " (p. 317). She concludes: "What Darwin was doing, in effect, was creating a 'logic of possibility.' Unlike conventional logic, where the compound of possibilities results not in a greater possibility, or probability, but in a lesser one, the logic of the *Origin* was one in which possibilities were assumed to add up to probability" (p. 317).

Miss Himmelfarb is being ironical in that last sentence. But in fact Darwin was not working with mere possibilities, but with what he tried to show were likelihoods, probabilities (that is what the mass of factual information in the *Origin* is all about), and the analogy with conventional logic is therefore inappropriate. For scientific proof of this sort ("convergence of evidence") *is* cumulative, and "unlike conventional logic" indeed, does add up to increased probability.

There are other, similar passages. Miss Himmelfarb describes Darwin's chapter on "Difficulties of the Theory" as "triumphing over" these difficulties by borrowing, for the weaker evidences, on the convincingness of the stronger, "insisting that anyone who had come with him so far could not rightly hesitate to go further" (p. 318). Thus, she says, he built an "argument from ignorance," in which future knowledge would be "bound to vindicate his theory" — and in this way even the awesome perfection of the human eye is "triumphed over" (pp. 317-319). But in fact this overlooks some very important points. For one thing, the *Origin*, far from simply insisting that one "could not rightly hesitate to go further," offers both plausible hypothesis and particular evidence for the evolution of the eye (*Origin*, pp. 168-171). Second, it is scientifically respectable, not of course to "triumph over" facts or to ignore negative results, but to hold to what seems a plausible, well-established hypothesis even if *some* facts seem for the moment recalcitrant. (Compare Mendeleev, for example, faced with the uncooperating atomic weights of certain elements.) And third, Darwin's tone is not that which Miss Himmelfarb implies in that

supposedly Darwinian proposition, "the future . . . would be *bound to* vindicate his theory" (my italics); Darwin's attitude is always appropriate to hypothesis: "The theory of natural selection . . . *seems to be* in the highest degree probable" (*Origin*, p. 434; my italics).

Still another objection must be made to Miss Himmelfarb's conception of scientific proof. She seems to assume that any evidence short of mathematical or experimental verification is necessarily suspect; thus she quotes Weismann in what purports to be a damaging admission: "It is not on demonstrative evidence that we rely when we champion the doctrine of selection as a scientific truth; we base our argument on quite other grounds" (p. 322). Darwin had himself complained of this sort of perverse treatment in late editions of the *Origin*:

> It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified. It has recently been objected that this is an unsafe method of arguing; but it is a method used in judging of the common events of life, and has often been used by the greatest natural philosophers. The undulatory theory of light has thus been arrived at; and the belief in the revolution of the earth on its own axis was until lately supported by hardly any direct evidence. (pp. 442-443)

Mr. Loewenberg remarks that Darwin "demonstrated the idea of evolution historically; he defined the concept of natural selection hypothetically" (p. 21). These are, again, scientifically respectable methods, as Darwin pointed out.

My fourth main objection is to Miss Himmelfarb's frequent, almost aggressive refusal to follow Darwin's mode of thinking. This is a reflection — or so I guess — of a different cast of mind from Darwin's, "categorical" (by which I mean tending to pigeonhole or categorize and to transform evidence into extreme instances) rather than "hypothetical" (by which I simply mean having scientific caution and the ability to maintain objectivity in the face of a hypothesis). She proposes, for instance, that Darwin's work was really an attempt to answer the teleological question, "why": "Why is life short? Why does the individual die, and why do species die? . . . [etc.]" (p. 155). But Miss Himmelfarb's long discussion of this question is only a steady blurring of the distinction between efficient and final causes; for "why" can have more than one meaning and therefore the wording of the question is not as significant as the method of providing the answer — and the answers in the *Origin* are to the question "how," not "why." "Darwin was able to give ultimate answers," Miss Himmelfarb says, "because he asked ultimate questions" (p. 156). But this is just what he did not do. Mr. Gillispie paraphrases Darwin when he points out, "Newton purports to unite his system with the principle of gravity, and Darwin with the principle of natural selection. But if either is asked what causes gravity, or what causes the variations that are selected, he does not know. Nor did the theory depend upon his trying to say. Indeed, its success hinged precisely on dropping that question" (*Forerunners*, p. 283).

It is perhaps this same reflection of the "categorical" mind going along in its own way that motivates Miss Himmelfarb's dismissal of Darwin's treatment of "perfect" organs (notably the eye). For the sake of brevity I only

urge the reader to observe the sensible case Darwin really makes for the evolution of the eye (*Origin*, pp. 168-171) and then compare Miss Himmelfarb: "Since the eye is obviously *of no use at all* except in its final, complete form, how could natural selection have functioned in those initial stages of its evolution when the variations had *no possible survival value*? No single variation, indeed no single part, being *of any use* without *every other* . . ." (p. 321; my italics). To have read the *Origin* or to have made even the most rudimentary study of the various types of animal photoreceptors seems to me to make such a misconception as this incredible. That Miss Himmelfarb could have made it is, I suspect, indicative of the "categorical" mind at work.

Her approving quotation from Bergson is similarly revealing: "If the accidental variations that bring about evolution are insensible variations, some good genius must be appealed [to] – the good genius of the future species – in order to preserve and accumulate these variations, for selection will not look after this" (p. 326). Ergo, teleology! Miss Himmelfarb does not reprimand Bergson for that silent but all-important substitution of the meaningless word "insensible" for the one Darwin almost invariably uses: "slight." Bergson, in making the substitution, attempts to reduce Darwinism to absurdity by reducing organic variations to such an extent that natural selection cannot work. But of course this is Bergson's natural selection, not Darwin's. Like Bergson Miss Himmelfarb tends to see Darwin's propositions not in his own carefully qualified terms, but in terms of the extreme instance; therefore she can juxtapose, in her arguments, not one organism with its immediate evolutionary antecedent, but creatures far removed in evolutionary progression – and thus can imply doubts about the evolution of the eye by natural selection, profess to see something absurd in Darwin's reference to dogs in discussing religion, and so on. All this is at the same level of understanding as Wilberforce's question to Huxley about his ancestry.

IV

"I think," Darwin wrote his father from the *Beagle*, "I shall be able to do some original work in natural history" (Himmelfarb, p. 75). Darwin was consistently the master of understatement; compare Huxley's later judgment: "there is no field of biological inquiry in which the influence of the 'Origin of Species' is not traceable . . ." (Loewenberg, p. 424; for detailed treatment of this point, see Sir Julian's introduction to the *Origin* [especially pp. x and xi] or Mr. Simpson's impressive account of Darwin's scientific impact [I, xiii]). One has to keep in mind, when students of Darwin point (as of course they must) to his various failings, that Darwin was *not* after all a great man because of his uncertainty about the inheritance of acquired characteristics or because of his theory of pangenesis; he was *not* great because of his Victorian valetudinarianism or his methodological amateurishness or because he did not fully appreciate, either in public or in private, the significance of his predecessors' work. Therefore a criticism which centers on these things is dealing with irrelevancies, something like Captain FitzRoy's skepticism about Darwin's nose, or Robert Darwin's paternal doubts about the shape of his son's head. Darwin's scientific greatness depends upon what Huxley immediately proposed that it

did: upon his having had that genuine insight about natural selection and upon having the patience and industry to assemble evidence in a more convincing way than had previously been done.

For the humanities Darwin's significance goes further. At the beginning of this essay I proposed three ways in which Darwin's work has shaped our view of the world and our thinking about it. These are, I think, necessary concomitants of life in a world that is not just evolving but evolving by natural selection – which is, as far as scientists have yet been able to tell, a correct conception. If I have spent what seems a considerable time opposing Miss Himmelfarb's and Mr. Lovejoy's contentions, it is because both of their works are certain to have influence and both of them are based upon what are, as far as scientists have yet been able to tell, incorrect conceptions. Furthermore, reviews of Miss Himmelfarb's book (like the front-page article in the 17 July *Times Literary Supplement*) have been generally bland or non-committal about all this; of those I have seen, only Donald Fleming's review in the *Journal of the History of Ideas* (June-Sept., 1959) makes a properly responsible objection.

Various essayists in this issue of *Victorian Studies* – but notably Mr. Passmore – have pointed out that scientific discovery influences men's thinking in general. Scientific progress makes a difference (to put it in terms which claim the least) as to what constitutes the domain of science and what is therefore left for philosophy to ponder. It makes a difference to one's thinking about philosophy, for instance, not only that man was evolved and not specially created but that he was evolved by natural selection, not by a vital force or cosmic urge of some sort. Darwinism and Neo-Darwinism are uncompromisingly non-teleological, non-vitalist, and non-finalist, and however "non-philosophical" this may be, it, also, affects the business of philosophers. Miss Himmelfarb's book attempts with bad evidence to refute this scientifically dominant point of view and Mr. Lovejoy's essay ignores it as a significant factor; this will of course make no difference to scientists, but it could be a real disservice to unwary students of the humanities. When these books are used in the classroom, therefore, as by their considerable merits they deserve to be, these reservations should be kept in mind.

Except for Miss Himmelfarb's book and Mr. Lovejoy's essay, the publications under review report this central issue clearly enough. Mr. Loewenberg in his introduction states its significance for man's place in nature: "Once man was swept into the evolutionary orbit, a revolution in Western thought was initiated. Man was seen to be a part of nature, and nature was seen to be a part of man. The Darwinian revolution was not a revolution in science alone; it was a revolution in man's conception of himself and in man's conception of all his works" (p. 21). A revolution so fundamental and a revolutionary so admirable do not often come together; the study of Darwin and Darwinism is therefore intellectually and even emotionally satisfying. To students of the humanities, as well as to scientists, it is clear that Darwin truly belongs to England's "band of gentle heroes."

P.A.

Indiana University

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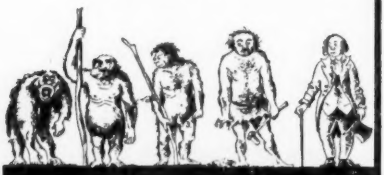

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